

233 45

Physico-Mechanical **EXPERIMENTS**

On Various Subjects.

CONTAINING

An Account of several Surprizing Phenomena

TOUCHING

Light and Electricity,

Producible on the *Attrition* of BODIES.

With many other Remarkable Appearances,
not before observ'd.

Together with

The Explanations of all the MACHINES,
(the Figures of which are Curiously Engrav'd on
Copper) and other APPARATUS us'd in making
the EXPERIMENTS.

By F. HAUKSBEE, F. R. S.

L O N D O N,

Printed by R. Brugis, for the AUTHOR; and Sold only
at his House in Wine-Office-Court in Fleet-street. 1709.

EXPERIMENT

Right Honourable

CONTAINING

JOHN

Lord Somers

Lord-President of Her Majesty's

most Honourable Privy-Council

May it please Your Lordship

I should be an idle Vainity to

me, to presume to

Lordship's Character; which is

universally known, that it needs not; and

so great, that it cannot receive any Addition



TO THE

Right Honourable

J O H N

Lord *Sommers*,

Lord-President of Her Majesties

most Honourable PRIVY-COUNCIL.

May it please Your Lordship,

I *T would be an Inexcusable Vanity in Me, to presume to attempt Your Lordship's Character; which is so universally known, that it needs not; and so great, that it cannot, receive any Advantage*

The Epistle

~~rage from the Descriptions even of the Ablest~~
and most Learned Pens.

Your Lordship will pardon my Boldness in making this Observation only, That under the greatest Weight of Public Business, and in a Multiplicity of Affairs of the highest Importance, wherein you have so eminently and so steadily promoted the true Interest of your Country; your Lordship has nevertheless, by the employment of a few vacant Hours, exceeded in Universal Learning, those whose Lives have been wholly spent in such Studies; and have been pleas'd to become such an Encourager and Promoter of all sorts of Knowledge, that no part of Learning has been considerable in our Age, without being Ambitious of the Patronage of your Lordship's Name.

Dedictory.

I am very sensible, how much the Imperfection of my Performance, and my want of a Learned Education, makes me stand in need of that Favour and Protection, which the Learnedst and most Accomplish'd Writers have been proud of aspiring to.

Particularly, the Honour your Lordship has done the Royal Society, in being some time their PRESIDENT; and the great Skill your Lordship is known to have attain'd to in the Works of Nature, no less Eminently than in the Knowledge of Men, and of the Nature and Ends of Government; have imbolden'd me to lay this small Attempt at your Lordship's Feet: Which having been already read and approv'd before the Society, may, with the Addition of your Lordship's Favour and Encouragement, hope to overcome all

The Epistle, &c.

*all the Disadvantages of coming from the
Hands of so undeserving a Person, tho', in
true Honour and Esteem for your Lord-
ship's Great Merit, not inferiour to any.*

I am,

My LORD,

With all Humility and Respect,

Your LORDSHIP's most Obedient

and Faithful Servant,

Fra. Hawksbee.

THE

THE
PREFACE.

THE Learned World is now almost generally convinc'd, that instead of amusing themselves with *Vain Hypotheses*, which seem to differ little from *Romances*, there's no other way of *Improving NATURAL PHILOSOPHY*, but by *Demonstrations* and *Conclusions* founded upon *Experiments* judiciously and accurately made.

By this course; after many Ages had pass'd, with little or no Progress in the *True Knowledge* of the *Nature of Things*, greater Advances have been made within the compass of a small number of Years, than was easily to be imagin'd, that the most *Sagacious Men*, with their greatest Industry, could ever have been capable of attaining to.

The P R E F A C E.

The Honourable and most Excellent Mr. BOYLE, by great Variety of Experiments, in almost *every part* of Philosophy, gave much Light into the Causes and Operations of Nature ; and particularly by the Invention of that most Useful Instrument the *Air-Pump*.

The Principal Subject of the following Papers is, an Account of *Great and Further Improvements* of this Noble Machin, the *Air-Pump*, and of many New Experiments made thereby.

By the same Method, the most Learned and Incomparable Sir ISAAC NEWTON has invented and establish'd the Theory of *Light and Colours* ; and by Demonstrations founded on Experiments and Observations, has at once begun and finish'd that great Discovery, and advanc'd that part of Optics, concerning the Nature of *Light and Colours*, of which there was little (if any thing) before known, to a *Perfect and Complete Science*.

The

The PREFACE.

The *New Experiments* contain'd in the following Treatise, concerning the Production and Emission of certain Kinds of *Light* from different Bodies, (hitherto unobserv'd) may, 'tis presum'd, give no small Illustration to that Matter; and become the Occasion of many not-unacceptable Discoveries, concerning several particular Circumstances not included in the general Theory.

The general Laws of *Attraction* and *Repulse*, common to all Matter, have by the same Excellent Person been discover'd and applied to Wonderful Purposes, in establishing the true System of Nature, and explaining the Great Motions in the World. But the *Nature* and *Laws* of *Electrical Attractions* have not yet been much consider'd by Any: And in the following Observations, 'tis hoped, the Reader may meet with many things, which may be of great Use in discovering some of the Wonderful and hitherto Unheeded Effects of this strange Property of Bodies, in several of the Operations of *Nature*; and possibly in the Production and Determination even of *Involuntary Motions* in the

The P R E F A C E.

Parts of Animals ; of which very little has yet been wrote intelligibly.

If the few Hints and Suggestions in this Discourse, shall excite the Curiosity of Ingenious Inquirers, to make further Search into these Matters, my Intention in publishing them, which was entirely for the Improvement of Natural Knowledg, will be compleatly answer'd.

F. H.

THE

T H E

CONTENTS.

A General Description of the Air-pump made use of in
the Experiments Page 1

S E C T. I.

An Account of several Experiments on the Mercurial Phosphorus P. 5

S E C T. II.

An Account of several Experiments made concerning the Attrition of Bodies, in various Mediums p. 17

Exp. I. Concerning the Attrition of Amber on Woollen in Vacuo p. 19

Exp. II. Concerning the Attrition of Flint and Steel in Vacuo. p. 21

Exp. III. Concerning the Attrition of Glass, and various other Bodies, in Vacuo. p. 23

1. On Glass and Woollen. ibid.

Varieties occurring in this Experiment, p. 25

2. The Attrition of Glass on Oyster-shells p. 27

3. The Attrition of Oyster-shells on Woollen, ibid.

4. The Attrition of Woollen on Woollen ibid.

Exp. IV. Concerning the Attrition of Glass on Glass, p. 32

Exp. V. Concerning the Attrition of Glass on Glass, under Water p. 35

An Experiment concerning the Production of a considerable Light, upon a slight Attrition of a Glass Globe exhausted of its Air. p. 36

An

The CONTENTS.

An Experiment concerning the Electricity of Glass, discovering it self in an extraordinary manner, upon a smart Attrition of it p. 42

A Continuation of the Experiments on the Attrition of Glass, p. 51

Some further Experiments relating to the Electricity of Glass p. 57

An Account of an Experiment, confirming the Production of Light by the Effluvia of one Glass falling on another, p. 65

S E C T. III.

An Experiment shewing the Difficulty of separating two Hemispheres, upon the injecting an Atmosphere of Air on their outward Surfaces, without exhausting the included Air p. 69

S E C T. IV.

An Experiment concerning the Proportion of the Weight of Air, to the Weight of an equal bulk of Water, without knowing the absolute Quantity of either. p. 74

S E C T. V.

An Experiment shewing that the Ascent of Liquids in small Tubes open at both ends, is the same in Vacuo as in the open Air. p. 77

An Account of an Experiment, concerning the quantity of Air produc'd from a certain quantity of Gun-powder fired in Common Air. p. 81

An Experiment about disturbing the Spring of the Air, p. 86

An Account of an Experiment, shewing the Cause of the Descent of the Mercury in the Barometer, in a Storm, p. 88

An Account of some Experiments made on the Phosphorus in Vacuo. p. 93

An Account of some Experiments made about the Propagation of Sounds in Condens'd and in Rarefied Air, p. 97
An

The CONTENTS.

- An Account of an Experiment, concerning the Resilition or Rebounding of Bodies, in various Mediums* p. 106
- Some farther Experiments concerning the Electricity and Light produc'd from various Bodies by Attrition,* p. 109
- Concerning the Electricity of Sealing-wax,* p. 114
- Concerning the Electricity, &c. of Sulphur and Rosin* p. 120
- Concerning some very Uncommon Effects of the Effluvia of Sealing-wax* p. 124
- An Account of the Success of an Attempt to keep several Atmospheres of Air condens'd in the space of one, for a considerable time* p. 127
- An Experiment concerning the Production of Light in an exhausted Glass (lin'd within-side with Sealing-wax) upon an Attrition made without.* p. 131
- An Account of several Experiments about the Ascent of Liquids, between the nearly-contiguous Surfaces of Bodies* p. 139
- Exp. I. Of the Ascent of Liquor between two Glass Planes in the open Air* p. 140
- Exp. II. The same in Vacuo* p. 142
- Exp. III. The Ascent of Liquids, between Marble and Brass Planes, in the open Air* p. 143
- Exp. IV. The Ascent of Liquors between two round Glass Planes, in the open Air* p. 144
- Exp. V. The Ascent of Water thro' a Tube fill'd with Ashes, in the open Air* ibid.
- Exp. VI. The Ascent of Water thro' Ashes in Vacuo,* p. 150
- Exp. VII. The Ascent of Liquors in small Tubes, of unequal Thickness, but equal Bores or Cavities,* p. 151
- Exp. VIII. The Ascent of various Liquors between two square Glass Planes* p. 152
- An*

THE CONTENTS.

- An Account of an Experiment concerning the different
Densities of the Air, from the Greatest degree of
Heat, to that of Cold, in our Climate* p. 170
EXPERIMENTS concerning the Refraction of the Air,
p. 175
*An Account of an Experiment concerning the different
Weights of the same sorts of Bodies (but of very une-
qual Surfaces) in Water, which were of equal Weight
in Common Air* p. 180
*An APPENDIX, containing some Remarks on the fore-
going Experiments.* p. 185
-

Physico.

Physico-Mechanical **Experiments, &c.**

*A General Description of the AIR-PUMP
made use of in the following EXPERIMENTS.*

THE AIR-PUMP (delineated Plate I.) consists of two Brass Barrels or Cylinders, as represented by *a a a a*, twelve inches in height, and two their diameters within. The Suckers, or *Emboli*, are rais'd and depress'd by turning the Winch *b b* backward and forward. The Winch is fasten'd to a Spindle that passes thro' a Lanthorn, whose Pins perform the Office of Cogs; for in its motion they lay hold on the Teeth of the Racks *c c c c*, and so reciprocally as one is depress'd the other is elevated: By which means the Valves, which are made of limber Bladder, and fix'd on the upper part of each *Embolus*, as well as at the bottom of the fore-mention'd Cylinders, perform their Offices mutually of exhausting and discharging the

B same

same Air taken from the Recipient on the Plate of the Pump. And when the Recipient comes to be pretty well exhausted of its contain'd Air, the pressure of the outward Air on the descending Sucker is nearly so great, that the Power requir'd to raise the other is very little more than what surmounts the friction of the moving Parts; which renders this Pump preferable to all other; for, in the Working of them, the nearer they approach a *Vacuum*, the greater is their Labour: But this that I am now describing (under the same circumstances) is quite contrary.

The bottom of the Barrels are plac'd in a Brass Dish, represented by *dd*, whose sides are about two inches high, and is on purpose to put Water in, to keep the Leather Collars (on which the Brass Cylinders stand) moist, whereby the Air is prevented from insinuating into the Cylinders in those parts. The Cylinders are screw'd down on the same by the Nuts *eeee*, which force the Frontispiece *ff* down on them, thro' which the two Pillars *gggg* pass. The Pillars have an Iron belonging to each of 'em, and pass from 'em in the form of a Swan-neck, decipher'd by *gg*, which Irons are fasten'd to the hinder part of the Frame, for their better security from shaking. From between the two Brass Barrels arises a Brass hollow Wire, *hhhh*, which hath a communication with each of 'em, by means of a perforated piece of Brass which lies along horizontally from one to the other. The upper end of this hollow Wire is fasten'd to another piece of perforated Brass, which screws on underneath the Plate *iiii*, which is 10 inches over, and has a Brass Rimm solder'd on it, to prevent the shedding of Water; for which

which there is occasion in several Experiments. Between the middle and the side of this Plate arises a small Pipe, *k*, about an inch and half in height, thro' which into the fore-mention'd hollow Wire passes all the Air into the Barrels, as it is taken from the exhausting Receiver. Upon the Plate of the Pump is always laid a wet Leather, on which the Recipients are plac'd: This wet Leather prevents the Air's getting into the Glasses, whose edges are truly ground, and is of use for that purpose beyond any Cement whatsoever, and not only secures it from the Air's ingress that way, but by the use of it we can make several Experiments in the same time they formerly could make one, without any daubing or difficulty. Another Excellency in this Pump is, the Contrivance of the Gage, denoted by *llll*, which Gage is a Glass Tube about 34 inches long, and is so plac'd that it cannot easily receive damage, and is altogether out of the way of any thing that is experimented on the Pump. Its lower Orifice is plung'd in a Glass of Mercury, describ'd by *mm*, on the surface of which is laid a piece of Cork with a hole in the middle for the Glass Tube to pass thro': On this Cork is plac'd a Board made of Box Wood, about an inch in breadth, and groov'd in the middle to receive the fore-mention'd Glass Tube, which is loosely loop'd on to the same by two Brass Loops, that it may have the liberty of rising and falling as the Mercury ascends or descends in the Gage. To the upper part of this Tube is cemented a Brass Head, which Brass Head fits into the fore-mention'd perforated Brass Piece that is screw'd on under the Plate, and has a communication as well with the Recipient on the same, as with the hollow Brass Wire *hhhh*

passing between the two Barrels. The Box Board is graduated into Inches and Quarters, from the surface of the Quicksilver to 28 inches high: from thence 'tis divided into Tenths of inches. By this Gage the Degrees of Rarefaction in any Experiment are at all times most nicely to be observ'd. The Air-cock, *n*, which lets in the Air, is likewise a Screw on the same fore-mention'd perforated Brass, in which the upper parts of the Gage and hollow Wire are inserted: *o o o o* represents a Receiver standing on the Plate of the Pump, on whose upper part *p p*, thro' a Box of Collars of Leather, passes a Slip-wire, whose Office is to take up, let fall, or suspend any thing at any determinate height, in the Receiver, without the Air's insinuation.

SECT.

S E C T. I.

An Account of several Experiments on the Mercurial Phosphorus.

EXPERIMENT I.

I Took a Glass Receiver, open and ground at both ends, and capable of containing about 30 ounces of Water: The upper Orifice of this Receiver was clos'd with a Brass Plate, (by the help of a wet Leather laid on the edge of it,) in the middle of which was screw'd a Stopcock, that had a small Glass Tube inserted into the lower Orifice of it; the Insertion was perform'd by means of a Cement: And the little Tube thus inserted, reach'd nearly from thence to the bottom of a Glass, which was also included within the 'foresaid Receiver, and which had as much Quicksilver in it as would cover the bottom of the Tube about a quarter of an inch. This *Apparatus* was then applied to the Pump, and the Stopcock turn'd, to hinder the Air's passage that way, till the Receiver was sufficiently exhausted: Which done, the Stopcock was turn'd again, to give the Air free liberty to enter in; and then the Air making its way thro' the Tube before mention'd, rush'd with a very great violence thro' the body of the *Mercury*, blowing it up forcibly against the sides of the Vessel that contain'd it. And in this confusion and hurry of its Parts it gave, all round, the appearance of Fire; it look'd like one great flaming Masse, compos'd and made up of innumerable

See Fig. 2.
Plate III.

ble little glowing Balls, which being forc'd up and dash'd against the sides of the Glass by the impetuous Torrent of Air, fell down again by their own weight into the rest of the Mercury. And thus the *bright* Phenomenon continued, till the Receiver was half fill'd again with Air.

The Result of this Experiment therefore, shews us, that *Light is producible from Mercury, by passing common Air thro' the Body of it, after the Receiver is well exhausted : i. e. that Light is producible by the application of a very subtile and penetrating Mover, to a Fluid of great density, whose Parts are most minutely divided, and of a smooth and polish'd Superficies, and plac'd where it has little disturbance, but from that Body which gives the Motion to its Parts.* For such a dense and polite Body, is Mercury; such a subtile Mover, is the Air; and such an apt Repository, is an Exhausted Receiver.

EXPERIMENT II.

HAVING provided a Receiver, of about 21 inches in height, I screw'd to the upper Orifice of it a Glass resembling those now commonly us'd for Cupping, having an open passage thro' its Neck, in which was cemented a piece of a small Tube, drawn tapering to one end by the Flame of a Candle: This, together with the Cup, made an entire Funnel, the small Aperture of which was stop'd with a round little Plug of Wood, to prevent the Mercury's entring the Receiver before its due time. Within this tall Receiver was included a Glass of the height

height of about 17 inches, which had a round Crown like a *Shade* (as they generally call those Fences which are put over Images to keep 'em from the Dust.) This whole *Apparatus* thus set together, See Fig. 3.
Plate III. was plac'd on the *Pump*, and about a pound and a half of Mercury put into the Funnel; and then working the *Pump*, by that time the Air had been drawing out for the space of two minutes, there was enough exhausted for exhibiting the Phenomenon intended. Having then loosen'd the Plug therefore, which stop'd the Funnel, the Mercury was driven by the pressure of the Air with great violence into the Receiver, and striking forcibly on the Crown of the included Glass, was thereby broken into very small Particles, and gave the surprizing appearance of a showre of Fire, descending all round the sides of the Glasses. The Light it gave in its descent, was such, that the form of the Receiver, and the Glass included therein, were both very distinguishable, and continued so to be, till all the Mercury had pass'd thro' the Funnel. All that space of time (I say) the Representation lasted; neither could any thing more lively express such a fiery Showre, than this descent of the Mercury in *Vacuo*.

What farther occur'd to Observation in this Phenomenon, I think proper to take notice of in some Particulars by themselves. I observ'd then,

That the descent of the Mercury resembled rather the fall of Snow, than that of Rain, by reason of the slowness of its motion.

That none of it appear'd luminous, but what was contiguous to the sides of the Glasses in its descent.

That

That the Globules of Mercury descended some swifter than others, according to their different magnitudes.

That the Mercurial Globules did not barely slide down along the sides of the Glasses, but were also turn'd about circularly; or, in other words, that besides their *motion of Perpendicular descent*, they had also a *Circular one about their own Axes*.

That the parts of the Mercury contiguous to the Glass, were by these Circumgyrations continually tearing and separating from their Contact with the Glass; and by that means were wrought up into such a form or shape, as was proper for the production of Light from such a body in such a *medium*.

That the smaller Globules, which adhered to the Glass, and whose weight were not sufficient to cause their descent, remain'd opake; for (in this, as well as all other Mercurial Experiments) no Light is to be obtain'd without Motion.

That the very same Motion as this was in *Vacuo*, given to the same Mercurial Globules in *Common Air*, will not produce the same Effect; Which I try'd, by condensing Air strongly on the Surface of Mercury, and so forcing that Mercury thro' Leather.

The Result of all which Observations put together, is, That a *peculiar Figure and Motion of Parts, as well as a proper Medium for those Motions to be perform'd in, are requisite to the Production of the Mercurial Phosphorus*.

Having since repeated this Experiment, and that with a *greater quantity* of Mercury; I have observ'd some particular Appearances in it, so remarkable
and

and surprizing, that I thought I ought not to pass 'em by in silence.

I made use of a quantity of Mercury, about *as great again* as what I us'd before, *viz.* near upon three pounds: And now the descending Mercury did not only appear like a Showre of Fire, (which it did at the first Trial) but also the Light darted thick from the Crown of the included Glass, *like Flashes of Lightning*, of a very pale colour, and easily distinguishable from the rest of the Light produc'd. These Flashes I have observ'd to be darted, sometimes *Horizontally*, sometimes *inclining upwards*, at other times *downwards*. And besides this difference with respect to the *manner* of the Reverberation of the Flashes, there was another thing observable with respect to the *Quarter* from whence they were so reverberated; for they would be thrown not only from the *included* Glass, but sometimes also from the *including* Receiver: And I have sometimes seen them rebound into Figures so very odd and surprizing, that I have no Idea of any thing that can serve for a just Comparison with 'em. But this is certain as to these strange Flashes, that they have sometimes seemingly *proceeded directly from the Stream of Quicksilver*, (as it descended from the Funnel) *before ever it reach'd the included Glass*; and that their general Course is, to *fly to the side of the outward Receiver*, where the Light breaks, and spreads it self into these odd forms. This is what they most frequently do. But if they take their original *from the side of the Receiver*, (as I have sometimes observ'd) then their Course is different.

Lastly, It deserves notice also, that during all the time of the Mercury's descent thro' the Funnel, (which was at least two minutes,) *the Crown of the*

included Glass appear'd to be sensibly more enlighten'd than all the rest of the parts of it: And this Light was uniform, and without any alteration, as long as the Quicksilver kept running.

EXPERIMENT III.

IN the foregoing Experiments, we have seen the Production of the Mercurial Phosphorus, in the fine and much-rarefied Medium of a *Vacuum*; by which 'tis plain, that such a Medium as that, is accommodate to the Nature of this Phenomenon, and will serve for the exhibition of it: But we can by no means infer from thence, that no other Medium will do; or, that Light, which may be produc'd in the rarefied Medium of a *Vacuum*, may not also be produc'd in a Medium less thin and rare than that: To bring this matter therefore to a determination, (*viz.* whether so thin a Medium as a *Vacuum*, or the nearest approach to it, be absolutely necessary to the production of such a Light, as is discoverable in the Barometer by putting the Mercury in motion,) I proceeded after the following manner:

The *Mercurial Gage* (an Instrument now universally known) I concluded would be the most proper Instrument for this Discovery. Having therefore plac'd a small *Receiver* upon the *Plate of the Pump*, the Air was exhausted from it, till the Mercury in the Gage was elevated to $29\frac{1}{2}$ inches; then suffering some Air to enter the Receiver by the *Cock*, the Mercury in the Gage descended, and made several

Vibrations before it reduc'd it self to a state of rest; and the Mercury having no other motion impress'd upon it in all these Vibrations, but barely that which the Air caus'd by its entrance, the Mercury all this time, tho' it did appear *luminous*, yet appear'd so only in the *Descents*, and not in the *Ascents*: But when the Mercury came to be broken and divided by a violent agitation and shaking, then the broken parts appear'd *luminous* in some part of their Superficies, the other part always being *opake*; and that after this manner: The *undermost* Superficies of the Mercurial Globules in their *Ascent* became *concave*, and there they were *luminous*; but the *uppermost* Superficies of the same Globules, in their *Descent*, became *concave*, and there likewise *luminous*: But the *uppermost* Superficies in the *Ascent*, which were *convex*; and the *lowermost* Superficies in the *Descent*, which were also *convex*, in both cases gave no Light at all, but continued always *opake*. And thus the Appearance continued upon every admission of Air, till near half the quantity that was exhausted was return'd again: But after that quantity of Air was admitted, then no manner of Light would ensue, tho' the Mercury had the same motion given it as before.

From this Experiment therefore we may draw the following Conclusion, *viz.* That tho' the Mercurial Phosphorus in the Torricellian Experiment is not produceable in so dense a Medium as common Air, yet it by no means requires so thin and so-much-rarefied a Medium as that which makes a near approach to a Vacuity. And this Truth receives a further Confirmation by the following

EXPERIMENT IV..

I Provided some Quicksilver, very fine, and free from the least appearance of soil on its surface. The Glass also which contain'd it, was made very clean and dry. I included this Glass with its Mercury in a Receiver on the Plate of the Pump, and exhausted the Air, till I found the Mercury in the Gage standing at 28 inches, and not above, (the Mercury in the Barometer at the same time being at $29\frac{1}{2}$ inches.) At this elevation of the Mercury in the Gage, the Pump was shaken, and by that means the Quicksilver in the included Glass was put into motion. *The Effect* of this Concussion was *the appearance of a Light*, bright enough to render the Receiver and the included Glass plainly visible and distinguishable; and not only so, but a Man's Hands and Fingers on the outside might easily be discern'd likewise. This was the Principal Matter of Fact enquir'd after by the Experiment; but I observ'd farther,

That tho' the Light might be produc'd by a small agitation of the Mercury, yet that Light would be encreas'd by a greater and stronger agitation.

That when a pretty brisk motion was given to the Mercury, it would give the representation of Waves of Light, breaking on the sides of the Glass, and scattering some Species of the same appearance towards the upper part of it.

That upon the repetition of the Experiment, the *Phosphorus* seem'd each time more vivid than the other; till at last, by often shaking the Quicksilver, its

its Surface became something soil'd, and so the Light less than it had been before.

That (in this, as well as all other Experiments on the Mercurial *Phosphorus*,) the Light exhibited is of a very pale colour.

That the first appearance of the Light, is when about half the Air contain'd in the Receiver is exhausted; and, That it still encreases with the encreasing Rarefaction of the Medium.

From this Account it appears, that the former Conclusion is again confirm'd, *viz.* *That there needs not the nearest approach to a Vacuum, to produce the Mercurial Phosphorus.*

And these two last Experiments put together, make up the fullest Proof of it that can be desir'd.

For in one of 'em the Air was *let in upon the Mercury* plac'd in the exhausted Receiver, and a Light was produc'd that way: In the other, the Air was *not totally remov'd from the Mercury*, and a Light was produc'd that way also. Now there can be no third way; and therefore 'tis universally true, that *the most rarefied Medium is not necessary to the production of this Phenomenon.*

EXPERI-

EXPERIMENT V.

THe Experiments hitherto related, have discover'd what Mercurial Lights may be produc'd, either in *Vacuo*, or in *Mediums* making some approach thereto. But the following one will shew what Light is produceable in a Medium very different from either of the former.

In order to this, I took a *Glass Globe*, (whose Content was about 30 ounces of Water;) and having put into it near half a pound of Pure Quicksilver, I clos'd the Mouth of it with a *Brass Cap*, which had a *Cock* inserted in the middle of it, by which means the Mercury had a free communication with the external Air: This done, the Globe was shook, and the Particles of Light presently appear'd in great plenty; they were of the bigness of small Pins heads, very bright and vivid, sparkling like little diminutive Stars in the *Lactes*, and exhibiting all together such a whitish appearance as those Stars do. Their number would encrease, according to the rapidity of the motion given the Globe; so that by proportioning the agitation, one might produce a greater or less number of these small Luminaries.

See Fig. 4.
Plate III.

Having carried the Experiment thus far; I took the same Glass Globe, with the same Mercury included in it, and applied it to the *Pump*, (by the help of a hollow *Brass Pipe*, which screw'd both to the *Cock* and *Pump*;) and then the Air being exhausted, and the *Cock* turn'd to prevent its ingress, the Globe was taken off, and moderately shaken. And now the *Phenomenon* was quite alter'd; for the Mercury appear'd luminous all round: It did not
now

now discover (as before) a Congeries of little, bright, twinkling Sparks, but a continued Circle of Light, which lasted all the time of the agitation. And if that motion were check'd with another of greater violence, it would then appear luminous almost all over the Globe. This being try'd, the Air was admitted again into the Cavity of the Globe, and then the Mode of Light return'd to its former appearance: The continued Circle of Light was lost, (neither could it be recover'd again by any making whatsoever,) and the little Stars return'd sparkling as before.

From what has here been related, we may infer,

First, That Light is produceable by the agitation of Mercury in the open Air.

Secondly, That this Light produceable in the open Air, is very different from that produc'd in Vacuo, or a much-rarefied Medium.

Thirdly, That the difference between these Lights, consists particularly in this; that the luminous Particles are **distinct and separate** in the one, and **united or blended** into one continued body of Light in the other.

Fourthly, That the Presence and Action of the Air is the cause of the separation of the parts of the Mercury into so many distinct luminous Globules, which in Vacuo form'd all one continued Circle.

And from hence it appears, that the Accounts given in this Experiment are not at all contradictory to any of the former, about the Mercurial Phosphorus; for there I speak of one kind of Light, and here of another. That Light which is produc'd in
VACUO,

Vacuo, or a very-much-rarefied Medium, is not the same with this produc'd in the open Air: And therefore when I say, that the *Mercurial Phosphorus* is not produceable, but in a Medium so or so qualified, it ought to be observ'd, that I speak there concerning a Light of the same kind and quality with that discover'd upon the agitation of the Mercury in the Barometer; for *that*, is vastly different from *this* which appears upon the shaking of the Mercury in the open Air.

From what has here been related, we may in-

fer, that the Light produc'd in the open Air,

is not the same with that produc'd in the open Air,

but is a very different kind of Light, and is produc'd in a

very different Medium, and is not the same with that

produc'd in the open Air, and is not the same with that

produc'd in the open Air, and is not the same with that

produc'd in the open Air, and is not the same with that

produc'd in the open Air, and is not the same with that

produc'd in the open Air, and is not the same with that

produc'd in the open Air, and is not the same with that

produc'd in the open Air, and is not the same with that

produc'd in the open Air, and is not the same with that

produc'd in the open Air, and is not the same with that

SECT.

S E C T. II.

An Account of several Experiments made concerning the Attrition of Bodies, in various Mediums.

The Description of the Machine for giving a swift Motion to Bodies in Vacuo, without admitting the External Air; represented by Plate II.

A Description of the *Air-Pump* (Fig. I.) being before given, I shall forbear taking any further notice of it, saving what immediately relates to the following *Experiments*.

A A A A **I**S a Ladder, such as is generally us'd in Houses.

B B Is a Bar of Iron, which passes through the middle of the upper Step, and is fasten'd to the Back-board of the Ladder by two Nuts and Screws thro' both the Board and Iron.

C C the Jaws of the Iron Frame which holds the great Wheel D D, of 23 inches diameter within its Groove.

E E the Brass Plate of the Air-Pump, on which the Recipient f f is plac'd.

g g The Spindle, to which Bodies of different magnitudes may be fasten'd, by a hole passing thro' the

D

the

the middle of them, sufficient to receive the Spindle; and by means of the two Nuts h h, a larger or a smaller Body may be screw'd fast on.

i i Is a Brass Plate turn'd true to the ground Edge of the Recipient on which it is plac'd, having a Brass Box in the middle of it, which is full of Collars of Leather well oyl'd, thro' which the Spindle passes; the hole of the Brass being likewise just fit to receive it.

k k k k Two Pillars, with Nuts to screw down a piece of Board, which has an Iron fasten'd to it to receive the upper point of the Spindle; the lower one falling into a Brass Socket, screw'd to the middle of the Plate of the Air-Pump.

L L L L The Supporters, reaching from the upper Board of the Ladder to the Pillars, to prevent the Recipient's being drawn from its Place by the motion and tugg of the Wheel-band.

m m The small Wheel, which the Band from the great one furrounds, and is one inch and half diameter.

N N The Winch which gives motion to the whole; the small Wheel m m making about fifteen Revolutions to one of the large Wheel D D; so that a Body fasten'd to the Spindle g g, of the small Wheel m m, will be turn'd fifteen times round to once of the great Wheel: And according as that shall exceed in diameter the small Wheel, so will the Velocity of the Motion of the extreme Parts be proportionably encreas'd.

o o o o A strong Board reaching from the lower Jaw of the Machine to the Ground, for the support, or giving a steady motion to the great Wheel.

p p Are two Screws, which fasten the Ladder to the Floor.

EXPERIMENT I.

*Concerning the Attrition of Amber on Woollen
in Vacuo.*

I Took some *Amber Beads*, of the bigness of small Nutmegs; and having pass'd a Thread through 'em, apply'd 'em by that means to a circular piece of Wood, which was *turn'd* with a *Groove* on the edge of it, on purpose to keep the Beads from being displac'd by the smart Friction they were to endure. Likewise, for their better security and fastness, there were so many *Pins*, or pieces of small Wire, driven thro' the Wood; and between every Bead there was a String ty'd over from *Pin* to *Pin*; the Beads, at the same time, standing out by the space of their Semidiameters beyond the body of the Wood into which they were fix'd.

In this manner was the whole put upon the *Spindle*, and made fast there by the two *Nuts*, (as was express'd before in the *Description* of the Machine :) Then the *Brass Plate*, on which the *Woollen* was wrapt, being screw'd to its place, (by means of the *Socket*, which receives the lower Point of the *Spindle*;) would spring back, and grasp the Amber with a moderate force. These things thus prepar'd, the *Receiver* was plac'd over them all together, with its upper *Plate* and *Box* for the *Spindle* to pass thro': The *Pump* then being set to work, the Mercury in the Gage was in a very little time elevated to about $29\frac{1}{2}$ inches; which shew'd the *Receiver* to be well

See Fig. 2
Plate V.

exhausted. . And now the great *Wheel* of the Machine being turn'd, the Amber had a very smart Attrition on the Woollen. At first, nothing remarkable appear'd ; but, in the space of a *Second* or two of time, there was a *Light which became sensible enough* : For where-ever the Attrition of the Amber was made ; while the Motion went on, there, and at all times, did the Light continue without intermission, and might be discern'd at three or four foot distance. Indeed it would not continue, if the Amber did any way desert the Woollen, notwithstanding the exceeding Velocity of the motion : But where the Attrition was uninterrupted, the appearance of the Light was so too, and propagated it self to that distance I have mention'd. And how swift the Motion was, which was given in order to the production of this *Phenomenon*, may be estimated from the Diameters of the several Revolving Parts ; for the Diameter of the great Wheel was 23 inches ; that of the small one mov'd by it, was $1\frac{1}{2}$; that of the Wood and Amber on the same Spindle with the small Wheel, was $4\frac{1}{2}$. Now, supposing the great Wheel to make two Revolutions in a *Second*, I think it follows, that the Velocity of the Extreme Parts of the Amber must be at the rate of a Mile in three minutes. And this violent Motion (as it may well be expected) was attended with Heat also, and such as discover'd it self by plain and sensible marks ; for the Amber (besides what the Touch discover'd) appear'd manifestly to be burnt and crack'd ; and the Woollen was not only discolour'd, but perfectly scorched, by the intense Heat.

I will conclude the Account of this Experiment with this one Remark, relating to the Light produc'd by this Attrition ; *viz.* That tho' the same Mo-

Motion and Friction was given the Amber in the *Open Air* as in the *Vacuo*, yet in the former case the Light was *very small and faint*, in comparison of what it was in the latter.

By this Experiment therefore we learn,

1. That Light is produceable in *Vacuo*, by the Friction of a solid Body (as Amber) against a Body of a soft and yielding nature, (such as Woollen.)

2. That this is not a meer lambent Fire, but such as is accompanied with a great Heat.

3. That this Light depends so immediately on the Attrition, as to disappear where that ceases.

4. That it requires a very thin and rare Medium, in order to its Appearance: And the thinner the Medium, the greater the Appearance.

EXPERIMENT II.

Concerning the Attrition of Flint and Steel
in *Vacuo*.

HAVING provided a Steel Ring about 4 inches diameter, and $\frac{1}{8}$ of an inch thick, I fix'd it between two pieces of Wood (of a less diameter) on the Spindle with the Nuts, as in the foregoing Experiment; the Edge of the Ring standing out beyond the Extremity of the Wood which held it, about half an inch. The Brass Plate (which I made use of for fastening the Woollen for the Attrition of the Amber) serv'd here also to fix a Piece of Flint, an Edge or Corner of which was placed towards the Steel:

Steel : And this Brass Plate, by vertue of its Spring, would hold the Flint pretty strongly to the Steel, notwithstanding some parts of it might be worn or chipp'd off by the rapidity of the motion : And in this manner 'twas cover'd with a *Receiver*, a *Brass Plate* and *Box* (as the former was).

Before any Air at all was exhausted, the great Wheel was turn'd, which gave a motion to the small one, and consequently to the Steel ; and by its collision with the Flint, there were Sparks of Fire produc'd in great plenty : But after some Air had been withdrawn, and the motion given, as before ; the Sparks which then appear'd, were neither so numerous as before, nor so bright and lively. And as more and more Air was still drawn out of the Receiver, so this Change in the Sparks produc'd, became more and more manifest. At every stop made, to repeat the Experiment in an higher degree of Rarefaction, I found the Sparks still to diminish, both in their Lustre and their Quantity ; till at last, when the Receiver came to be well exhausted of Air, there was not one Spark to be seen, tho' a much-greater motion was given than before, and consequently a more strong and valid collision of the Flint and Steel. All the Appearance in this case was, only a faint, continued, little Streak of Light, visible on the edge of the Flint that was rubb'd by the Steel.

This being try'd, there was then some Air let in to the Receiver, upon which (the motion being given as before) some Sparks were discover'd, but of a dull gloomy hue : Upon the letting in a little more Air, by I know not what accident, the whole quantity of Air forc'd it self in, and then the Wheel, being set to work again, the Sparks appear'd as numerous and as vivid as at first.

The

The Conclusion therefore from this Experiment is,
*That the Air's Presence is absolutely necessary to that vi-
 gorous expansive Motion of the Parts of Bodies, wherein
 the Nature of Culinary Fire consists.*

Qu. Whether the Light visible on the edge of the
 Flint, when the Receiver was well exhausted, was
 not of the same (*lambent*) kind with other Lights
 produc'd by the Friction of certain Bodies, of which
 mention is made in some of our Experiments?

EXPERIMENT III.

*Concerning the Attrition of Glafs, and various
 other Bodies, in Vacuo.*

1. *Concerning the Attrition of Glafs and Woollen.*

I Took a *Glass Globe* of about 4 inches diameter,
 having a Passage thro' the middle of it to re-
 ceive the *Spindle*, which was fasten'd to it with
Corks and *Screws*. The *Woollen* against which the
 Friction was to be made, was the coarsest sort of
 that which is commonly us'd for *Gartering*, which
 I chose purposely on the account of its harshness,
 as being likely to improve the Phenomenon to a
 higher degree than the *Cloth-Lift* I had us'd before.
 This was wrapt about the Arms of the *Brass Spring*,
 and being screw'd down to its place, gently em-
 brac'd

brac'd the Globe: Then a large *Receiver* was put over all, and the *Pump* being set to work, in a little time the Air was exhausted out of the Receiver. The great Wheel being then turn'd, gave fifteen Revolutions to the included Globe, at each of its own: Which swift Motion giving a smart Attrition on the Woollen, quickly produc'd a beautiful Phenomenon, *viz.* a fine *purple* Light, and *vivid* to that degree, that all the included *Apparatus* was easily and distinctly discernable by the help of it. And thus it continued while the Friction lasted.

Upon the *letting in* a little Air, the *Light* and the *Colour* were both chang'd; the Light impair'd in its *Brightness*, and the Colour in its *Tincture*. And as the Air, at several times, was suffer'd to return into the Receiver again, so did the Light still become more *pale* and *faint*; tho' even when the Receiver was quite fill'd with Air, some *feeble Light* would still discover it self, upon the same Attrition given as at first.

I found, that this Purple Light was *visible no where*, but *only on the Arms of the Brass Spring*, where the Glass in its motion rubb'd upon the Woollen; and, that the Dimensions of it were about half an inch in breadth, and one inch in height: And farther, That it did all the while remain steddy in its position, without any the least Undulation, tho' the motion of the Glass was so considerably swift.

Some

Some Varieties occurring in the Experiment last mention'd, at various Trials.

When this Experiment came to be repeated two or three times, with the *same* Glass; no *Purple Light* would appear, but a *Pale* one only succeeded in its room; neither could I recover the *Purple* with that *same* Glass, by any methods whatsoever that I could use.

When I took a *fresh* Glass; after I had made use of it (this way) two or three times, the *Purple* was lost again, and could not be retriev'd. And if a new one were us'd, the Effect would still be the same.

Sometimes, if the Glass were taken out after a violent attrition, it would be so hot, as sensibly to offend the Hand that held it: And the Woollen would appear not only discolour'd, but perfectly burnt through.

Sometimes the Light would not be confin'd to those strict Bounds already mention'd, but would be spread quite round the Globe, and make an entire continued Circle, all the time of the motion; notwithstanding that it touch'd the Woollen in no more parts, than it did in the former Experiment.

Sometimes a perfect distinct Halo would appear, spreading it self quite round the fixed Light. This I attribute to some Particles of Water, insinuating themselves, by the Spindle, thro' the Box on the upper Brass Plate (where Water is always kept, to prevent the entrance of Air in that place:) For this Water descending along the Spindle, till it reach'd some part which was of a greater extent, would there (as I conceive) by the violence of the motion given, be thrown all about the Receiver in

E small

small drops; some of which being very likely to fall on the *Woollen*, would there be *heated* to a considerable degree by the attrition of the *Glass*; and being consequently *evaporated*, would appear there in the form of a *Halo*, surrounding the Light. And what confirms this Solution, is, That having since form'd a Contrivance to prevent the scattering of the Water, no such appearance of any *Halo* has been observ'd. But to proceed,

In this Experiment I have shewn the Effects of the Attrition of *Glass* on *Ordinary Woollen*; I would now add an Experiment concerning the Attrition of *Glass* on *Woollen* some way prepar'd or qualified beforehand.

I took some of the 'foremention'd *List of Cloath*, which had been drench'd in *Spirit of Wine*; and fasten'd it to one Arm of the *Brass Spring*: And some of the same *List*, which had been steep'd in Water impregnated with *Salt-petre*, I ty'd to the other Arm of the said Spring: But both pieces were well dry'd before I made use of 'em.

Then, upon the Attrition, I observ'd the Light to break from the agitated *Glass* in a very odd form, resembling that of *Lightning*. This is manifestly different from the last Phenomenon: For there indeed we had a *delicate Purple-colour'd Light*; but here, a brisk *fulgurating Light*, scattering it self about in Flashes, and darting with a force from the surface of the revolving *Glass*.

2. *Concerning the Attrition of Glafs on Oyfter-shells.*

Instead of the 'formention'd *Woollen*, I made use of two flat *Oyfter-shells*, well dry'd: Each Arm of the Brass had one fix'd to it. Upon the usual motion given, a Light appear'd, resembling a *fierce-flaming Spark*, just upon that very spot where the Glafs and the Shells touch'd one another. This Light did not dilate or extend it self, but kept within the bounds where it first appear'd; and it was but a small compass that it appear'd in.

3. *Concerning the Attrition of Oyfter-shells on Woollen.*

The Success of this Trial was, that it produc'd a Light, but an *obscure* and *dim* one, and, at best, like a faint *Halo*.

4. *Concerning the Attrition of Woollen on Woollen.*

I took some of the *Lift* formerly mention'd, and bound it about the edge of a *Wooden Wheel*, which I had caus'd to be *turn'd* for that purpose. This Wheel (with its *Round* of *Woollen*) I fasten'd on the *Spindle*; and some of the coarse *Gartering* was also put about the Brass *Spring*. The Result was, that upon the motion given (as usual,) a *small glimmering* Light appear'd, but such as gave no prospect of *being any way improv'd* by the continuance of the motion: The *Woollens* were not in the

least discolour'd, tho' the Friction was sometimes more than moderate; neither was there any sign at all of such an Effect, as hath been shewn to be produc'd by the Attrition of Woollen on other bodies: The Light (which had been produc'd) *totally disappear'd* upon the re-admission of less than a quarter part of the Receiver's natural content of Air, tho' the Attrition made then was as great as it had been at any time before.

'Tis further to be observ'd, that I could never find, that the *different Colours of Woollen* contributed any thing to the *different Colours of Light*, exhibited in any of these Experiments.

The several Particulars of Fact which we learn from these Experiments, are reducible to the following Heads:

That a Purple Light was produc'd by the Attrition of Glass on Woollen (in Vacuo.)

That the Purple Light decay'd, both as to its Tincture and Vigour, upon the admission of the Air; and did more and more do so, as more Air was let in.

That this Purple Light is lost, after two or three successive Trials with the same Glass.

That the bounds to which the Purple Light confines it self, are of different and various extents; reaching sometimes all round the Globe, and at other times being only about the place of the attrition; but still steady, and without Undulation.

That a brisk Fulgurating Light was produc'd by the attrition of Glass, on Woollen impregnated with saline and spirituous parts.

That a Red Fiery Light was produc'd upon the attrition of Glass on Oyster-shells, which Light was confin'd within

within a narrow compass, and did not spread it self farther about.

That a faint dim Light was produc'd, upon the attrition of Woollen on Oyster-shells.

That a small glimmering Light was produc'd, upon the attrition of Woollen on Woollen; which was not encreas'd by the continuance of the Motion, and which disappear'd upon a small admission of Air.

That in the attrition of Glass on Woollen, not only a Light, but also a great Heat was produc'd; discoverable by the Glass, and its Effects on the Woollen too.

And from these Observations we may make the following Remarks.

First, *That different sorts of Bodies afford us remarkably-different Lights, different in Colour, and different in Force and Vigour. This appears from the particular matters of fact now set down.*

Secondly, *That the Effects of an Attrition may be various, according to the different preparation and management of the Bodies which are to endure it. Thus the Woollen, tinctur'd with Salts and Spirits, gave such a Friction, as produc'd quite a new sort of Light.*

Thirdly, *That Bodies which have yielded a particular Light, may be brought by Friction to yield no more of that Light.*

This is plain, from the *strange and surprizing loss of the Purple Colour*, after two or three Trials made with the same Glass; for that *Purple* could not be recover'd by any Art or Means whatsoever.

A *Pale Light* indeed was produceable from the same Glass afterwards, but the *Purple* was irretrievably lost: Therefore the *Purple-colour'd luminous Matter* must either be quite spent (that is, evaporated.

vated and carried off) by the Attrition, or else the Configuration and Texture of the Glass underwent such a change by that same Action, that it was no longer capable of admitting the Purple Light.

If the Cause were from the Consumption of the Matter, then we may reason thus; That that certain determinate quantity of colour'd-luminous Matter, which is lodg'd in some Bodies, may, by a Course of repeated Attritions, be quite exhausted and carried away.

Further; because a Pale Light is produceable after the other is gone, we may likewise upon the same Supposition argue thus: That some sorts of colour'd-luminous Matter are more easily separable from Bodies than others; or, That they require different degrees of Force and Friction, in order to their evaporation and discharge.

And this is highly reasonable, since the Parts of differently-colour'd Lights are undoubtedly of different magnitudes and bulks; and consequently, That Force which may be sufficient to put some of 'em into vibratory expansive motions, may not be sufficient to produce the like Effects upon others.

On the other hand, if the loss of the Purple was owing to the alter'd Tone or Texture of the Glass, then the Conclusion will be, That Bodies may be so chang'd by proper degrees of Friction, as to retain some sort of colour'd-luminous Matter, which they did once emit (as here the Purple;) and to emit another sort, which they did at first retain; (as in our Case the Pale Light.)

Now, in favour of the latter of these two (and, as I take it, the only two possible) Causes, it may be alledg'd, That the next Experiment will prove to us a Diminution of Light, or a Decay of its Force and Colour, consequent upon an Attrition. But then, as we shall see, those Bodies are both hard and inflexible,

flexible, such as, by rubbing, would wear and alter one another's Texture : And besides, the Phenomenon there, is only a *diminution or decay of the Strength and Lustre of a Colour* ; a meer *gradual alteration of the same Colour*, not the production of a *new and different* one. On the contrary, in this Phenomenon now before us, one Colour is *lost*, and a very different one appears in its room : And the Attrition here us'd, was that of a *soft and yielding Body* against a *hard and inflexible* one ; so that the *wearing* of the Parts, and the destruction of their *Spring or Tone* consequent thereupon, is not here so easily conceivable. However, upon the whole, I shall leave it as a

Query. Whether the *loss* of the Purple Colour be owing to the *Consumption* or the *Retention* of the matter ?

EXPERI-

EXPERIMENT IV.

*Concerning the Attrition of Glafs on Glafs.*First, In *Vacuo*.

I Took a *Glass Globe* about three inches in diameter, which I fix'd to the *Spindle*; and to the two Arms of the *Brass Spring* were ty'd two *slips* of *thin Board*, which had pieces of a *Glass Tube* fasten'd to 'em, by the means of some small *Neal'd Wires* which were put thro' their cavities. These Wires likewise pass'd thro' some holes in the Board, made for that purpose; and so kept the pieces of Tube firm and tight in their places. All this was cover'd with a large *Receiver* (as usual); and the *Pump* being wrought, the Air was exhausted.

See Fig. 6.
Plate III.

The great Wheel then being turn'd, a swift motion was given to the included Globe; and by the friction of this on the 'foremention'd Tubes, a *considerable Light* was produc'd. The whole included *Apparatus* was perfectly discernible by it; and the Illumination had been much greater, had not the Daylight prevented. (For it was then but a little past Five P. M. in the Month of *February*, the Horizon being clear, and the Room open wherein the Experiment was made.) The Colour of the Light produc'd, resembled that of *melted Glass*: And, which is very remarkable, this glowing Colour did not appear only just upon the *Parts where the Friction was made*, but also at the *Extremities* of the Tubes, where the Globe did not touch 'em.

Secondly,

Secondly, In *less-rarefied* and *Common* Air.

THE Air being suffer'd to enter the Receiver by degrees, and at several times, the Motion was given at each of those Ingresses of the Air: And the Phenomenon here observ'd was, that *no sensible decay of Light or Colour was to be discover'd*, at any of those times: Except only that at last, when the Tubes by much rubbing were worn, and consequently their Spring lessen'd and impair'd, then indeed the Light would be diminish'd, in proportion to the abatement of their force on the moving body. And this I have often observ'd, when the motion has been made for some time in *Vacuo*, or in *Common* Air. So that I question not, but if the Experiment had been begun where it ended, (*in this Trial*,) the lesser Light would then have been exhibited in *Vacuo*. (That is, had the Experiment been made, even in *Vacuo*, with Tubes thus rubb'd and worn, and consequently their Spring weaken'd, a less Light would have been produc'd, than if the Experiment had been made in *Pleno*, with Tubes not thus weaken'd and impair'd by the attrition.)

NB. I have since tried this same Experiment in *Vacuo*, and that about *Noon*, in a clear day. And I found, that the Light was even then as sensible, as that of a piece of *Red-hot Glass*, of the same bigness, would have been in the open Air. Yet this appearance, as discernible as 'tis, *continues not any longer than the motion is continued*.

P

By

By this Experiment we learn,

First, *That a considerable Light is produceable by the Attrition of Glass on Glass, (that is, of some sorts of hard Bodies, on others of the same kind) in Vacuo.*

Secondly, *That this Light continues unalter'd upon the admission of Air.*

Coroll. *We may see the difference therefore between this Light here produc'd, and those mention'd in other Experiments: For this endur'd without change the shock of the returning Air; whereas most or all of those formerly recited, underwent several gradual alterations, according to the degrees of the Air's admission.*

Thirdly, *That when the Tubes were worn by the Friction, there was an abatement of the Light: and the more wearing, the more abatement.*

Coroll. *We may see from hence, how much the due Spring and Tone of the Parts contributes to the emission of Light, in those Bodies which will emit it. So that we may reasonably believe in general, that very great alterations may be wrought in Bodies as to their Luminous qualities and properties, either lessening and destroying them on one hand, or perhaps encreasing and improving them on the other by the actions of other contiguous Bodies upon them, according as those actions tend, either to the weakning or confirming the Spring and Tone of their Parts.*

I have only two Observations to make farther, with regard to this Experiment.

First, *That the Glaze or Polish on the surface of a Glass, adds nothing to the Light, as far as I can find by any Observation.*

Secondly, *That tho' this Colour be like that of red-hot or melted Glass; yet the Glass is not really in such a state as to be red-hot, because this Colour never outlives the motion, but is gone presently upon the ceasing of it; (as has always been observ'd, in the darkest Night:)*

Night :) whereas if 'twere really red-hot with the motion, the appearance must necessarily be preserv'd for some small time, at least.

EXPERIMENT V.

*Concerning the Attrition of Glafs on Glafs,
under Water.*

THIS is no more than a Repetition of the last Experiment in another Medium ; for the whole Apparatus (there mention'd) was now entirely immers'd in *Water*. Upon the first Friction of the Globe on the Tubes, a pretty brisk *Light* was produc'd, which inlighten'd the whole Body of the *Water*. The parts of the Tubes, where the Friction was made, were distinguishably *Red*; but the appearance soon began to lessen, and in no long time quite died away. For the *Water* grew thick and turbid, by the Grit or Powder of the Glasses, which was worn off by the Attrition. It approach'd continually nearer and nearer to the Colour of *Whey*, so that the *Light* could be but just discover'd, as glimmering through the body of it; and that not continually neither, but with interruption, and like faint *Flashes* sent out at a distance. Tho' at some other trials, I have observ'd the *Light* to be more considerable than it was at this time.

The *Dust* rubb'd off by the Attrition of the Glasses, I view'd thro' a good *Microscope*; the Particles of it appear'd to be of a long and slender figure, but I cou'd not discover the least sign of their having been any way in *Fusion* or melted.

We see therefore, that Light is produceable by the Attrition of *Glass* on *Glass*; not only in *Vacuo*, and *Common Air*, but also in *Water* too. And hence 'tis evident further, that the *Glasses* are not heated red-hot by the Attrition; whatever such resemblance the Colour carries along with it.

An Experiment concerning the Production of a considerable Light, upon a slight Attrition of a Glass Globe exhausted of its Air.

I Took a Glass Globe of about 9 inches diameter, and exhausted the Air out of it; then (having turn'd a *Cock*, which prevented the return of the Air) I took it from the *Pump*. The Globe being thus secur'd, I fix'd it to a Machine, which gave it a swift Motion with its Axis perpendicular to the Horizon: and then applying my naked Hand (expanded) to the surface of it, the result was, That in a very little time a considerable Light was produc'd. And as I mov'd my Hand from one place to another (that the moist Effluvia, which very readily condense on the Glass, might, as near as I could, be thrown off from every part of it,) by this means the Light improv'd; and so continued to increase, till words in Capital Letters became legible by it: (as has been observed by Spectators.) Nay, I have found the Light produc'd to be so great, that a large Print might without much difficulty be read by it: and at the same time, the Room, which was large and wide, became sensibly enlightned, and the Wall was visible at the remotest distance, which was at least 10 Foot. The Light was of a curious Purple Colour,

Colour, and was produc'd by a very slender touch of the Hand; the Globe at the same time being scarce sensibly warm: neither could I ever find, that a more violent Attrition did contribute any thing to the encrease of the Light.

Now after this Attrition of the exhausted Globe had been continued for some time, the Cock was turn'd, which gave liberty to the Air to enter into the Globe through the Joynts of the Screws; the motion of the great Wheel, and the application of the Hand, continuing all the while as before. Then I observ'd, that as the Cavity of the Globe became more and more replenish'd with Air, so the mode of the Light continued to alter, till the same quantity of Air was re-admitted, as had been exhausted. And when such a quantity was once enter'd, there was then as great a difference between that Light and the Light produc'd when the Globe was empty of Air, as between the Lights produc'd from Mercury, when the Experiment was made in *Vacuo* and the open Air. For if a Man touch'd the Globe with his Fingers, there were specks of Light (tho' without any great Lustre) seen to adhere to them. Nay, while my Hand continued upon the Glass, (the Glass being in motion,) if any Person approach'd his Fingers towards any part of it in the same Horizontal Plane with my Hand, a Light would be seen to stick to 'em, at the distance of an inch or thereabouts, without their touching the Glass at all; as was confirm'd by several then present. And 'twas observ'd also, that my Neckcloth, at the same time, at an inch or two distance from the Globe, appear'd of a fiery Colour, without any Communication of Light from the Globe.

This

This was the Event of this Experiment, at the *first time* of making it; in which case the Air was let into the exhausted Globe *all at once*.

But at the *second time* of making this Experiment the Air was not all let in at once (as before,) but *gradually*, and *at several times*; by which means the Modes of Light produc'd in the several different Mediums was the better observable: Tho' it must be own'd, that here was no great alteration of the Light, either as to its Vigour or Colour, till so considerable a quantity of Air was let in, as amounted to more than one quarter part of the Globe's natural Content. But many times, before half its Content (as near as I could guess) was let in, the Light began to branch it self into pleasant Figures, from that side of the Globe touch'd by the Hand; so that the whole body of the Globe was fill'd with these fine Appearances. And as more Air was admitted, so the Stems of these Branches of Light became more and more slender and minute, striking then also against the opposite side of the Glass, and rebounding from thence in a manner very surprizing, and delightful to behold; till at length, more Air still being let in, the Light and the Figures both diminish'd, and continued so to do, till the Appearance became the same as was related at the conclusion of the first Trial.

I would only note here, That what difference soever there was, as to the latter parts of these two Trials, the former parts of both were alike; except only, that in the second Trial, upon the application of a piece of white Sheeps-leather, a good Light was produc'd while 'twas held to the Globe with the Wooll-side next it; but when 'twas turn'd
with

with the other side to the Globe, tho' it was continued thus expos'd for some time, yet no Light appear'd: But then turn it again, and the same Light would appear as at first. And thus it happen'd upon several times repeating the same.

Having taken notice (in the Experiment now recited) *that the highest degree of Rarefaction of the Air in the Globe, is by no means necessary to the production of this Light, since it would be very little less'n'd, either as to Vigour or Colour, till (perhaps) more than one fourth part of the Air was let in:* I would add further, That I have often observ'd the same thing, as to the Light produc'd in the *Mercurial Experiments*; tho' the Colour indeed was not the same, for in those Experiments it was always pale. And since in those Experiments with Mercury, there is such a seeming congruity of Appearances, in all circumstances, with those made on the Attrition of Glass without it; one might conjecture with some probability, that the Light produc'd proceeds from some Quality in the Glass, (upon such a Friction or Motion given it,) and not from the Mercury upon any other account, than only as it is a proper Body, which, by beating or rubbing on the Glass, produces the Light. And that which seems to back such a Conjecture, is, that having rubb'd the upper or empty part of a Mercurial Barometer between my Fingers, a Light follow'd upon it without any motion of the Quicksilver. But notwithstanding all this, the matter is doubtful; and there may (for all that we know) be a Luminous Quality in Mercury, as well as in Glass or other Bodies; which the following Experiment (purposely made) seems to countenance.

I took

I took a small quantity of Quicksilver, and put it into a Galley-pot, wherein Varnish had often been us'd, by which means the Pot was pretty well lined with it. The Weather at that time was *moist*, which influenc'd the Varnish so far, as to soften it a little. Now when this Galley-pot, with its contain'd Mercury, came to be in *Vacuo*; as soon as the Pump was shook, a Light appear'd: and this without any concurrence of Glass, or the assistance of a more proper and favourable Season. I purposely mention the moisture of the Weather, because a humid Air would sometimes render the Experiment unsuccessful, even in Glass, or at least take off very much from the Appearance of it.

Farther; I am inform'd by several Persons of Credit, That *Mercurius dulcis*, if broken in the dark, will yield notable Flashes of Light. But since the Mercury in that Preparation is pointed with Salts, (and each Globule of it consequently wrapt up in the same,) a Man can't be sure that the Salts do not contribute to the Phenomenon: For I have often observ'd, that Loaf-sugar, when struck or broken in the dark, affords a Light; and I can't tell but Salts, as closely united in their parts as Sugar, may, upon a violent separation of 'em, do so likewise: But this I intend to enquire into by some farther Trials, *viz.* *First*, I design to try whether *Mercurius dulcis* will afford any Light, when broken in *Vacuo*; since, if it proceeds from the Mercury, and there be such a quality in that Body, 'tis highly reasonable to expect it then, so rare a Medium being the most proper to discover it in. *Secondly*, What the Salts will do without Mercury, both in the open Air and in *Vacuo*; for there are some Bodies, which appear luminous in the open Air, and yet totally lose

lose that quality in *Vacuo*. As for instance; I took a piece of Wood, (which I suppose had lain underground a considerable time,) very moist, but not rotten. In the dark it appear'd very vividly of the colour of Fire: But having inclos'd it in a Receiver on the Pump, I found that as the Air was withdrawn, the Fire-like appearance proportionally decay'd; till at last in *Vacuo*, it became perfectly void of Light; and then, as the Air was let in again, so it recover'd its first brightness. This I repeated several times with the like success.

But to return now to the Experiment. The matters of Fact to be observ'd from thence, are reducible to the following Heads.

The *Production of a Light* by the *Friction* of the Hand on the surface of the *Exhausted Globe*.

The *great improvement and encrease of this Light*, by the *motion of the Hand* from one place to another.

The *slightness of the Friction* requisite to produce this Light; and its not encreasing by a more *violent* one.

The *Alteration of the Light*, upon the *Re-admission* of the Air.

The *Continuance of that Alteration*, as more and more Air was admitted.

The *Light's communicating it self to Bodies plac'd near it*, when the whole quantity of Air drawn out was re-admitted.

This, when the Air was let in *all at once*.

But when the Air was let in *gradually*; then *no great change of the Light*, as to colour or *vigour*, till more then $\frac{1}{4}$ of the Globes natural content of Air was admitted.

The wonderful *Figures, Branchings, and Reverberations* of the Light, as more Air was let in, till it came to the bounds of a certain quantity of Air admitted, where the appearance was at the height.

The *gradual diminution both of the Light and Figures*, (after it was come to that limit,) upon the admission of more and more Air.

An Experiment concerning the Electricity of Glass, discovering it self in an extraordinary manner, upon a smart Attrition of it.

HAVING procured a Tube, or hollow Cylinder, of fine *Flint Glass*, about one inch diameter, and thirty in length; I rubb'd it pretty vigorously with *Paper* in my Hand, till it had *acquir'd some degree of heat*. I then held it towards some pieces of *Leaf-Brass*; which were no sooner within the Sphere of Activity of the *Effluvia* emitted by the Tube, but they began to be put into brisk motions, and yielded the following surprizing appearances.

They would leap towards the Tube, at a very considerable distance from it; nay, I have found, that sometimes the distance of 12 or more inches has not prevented their doing so.

Sometimes they would adhere and fasten to the Tube, settling themselves on its surface, and there remain quiet: and sometimes they would be thrown off from it, with a very great force, even to the distance of 6 or 7 inches. And not only when they adher'd to the surface of the Tube, would they thus suddenly and precipitantly be driven from it; but also in their motion of ascent towards it, even when they were advancing

vanc'd so far as to touch the Tube, this repellent force would take place, and hurry them *downwards* with a great velocity.

And (which still adds to the Wonderfulness of the Phenomenon) they would often *repeat this alternate rising and falling*; the *Attractive* and *Repulsive* forces (whatever they are,) exerting themselves as it were by turns; the one drawing up, and the other beating down these light bodies; and that for several times one after the other.

Neither is this all the variety which the Phenomenon afforded; for sometimes they would *move but slowly* towards the Tube, sometimes they would *remain a small time suspended, between the Tube and the Table* on which they were first laid; and at other times (which is no less strange than the former) they would *seem to slide along in the Direction of the sides of the Tube, and that without touching it.*

But besides these, there are yet some other Observations relating to this Experiment, which I think not amiss here to mention.

First, The hotter the Tube was made by rubbing, to the greater distance did the attractive force extend itself. But that it will answer in proportion to any degree of heat excited, is what I will not venture to determine. I mean; whether, encreasing still the degree of heat in the Tube, the Sphere of the attractive Power will still be proportionably enlarged; or whether there may not be some certain degree of heat, which may carry the attraction to its utmost limits of distance, so that all degrees above that, shall produce less powerful effects this way: Whether this be so or no, (I say,) is a point which will require farther Trials, and more Consideration, in order to a compleat decision of it.

Secondly, Tho' all the various appearances now mention'd, as to the motions of the attracted little Bodies, do not happen at every Trial, yet many of them are observable at all times; neither are there any hinted, but what I have at some time or other punctually observed. And the reason of this difference, seems very probably to arise from the different Temper and Constitution of the Air. For when the Air is clogg'd, either with *humid and aqueous*, or other *more gross and solid parts*, rais'd up from the vast Fund of Terrestrial matter here below, there's no doubt but the resistance these fine Effluvia then meet with in their way, must be much greater than when the Air is free, and no such Impediments lie to oppose them in their passage. For the Effluvia, how subtle soever they can be imagin'd to be, are yet *Body and Matter*, and must therefore be liable to the common Laws of Bodies, which is to be resisted in some proportion to the strength and density of the *Medium*. Neither is it improbable, but that in a moist Constitution of the Air, the watery parts may run together, and condense on the surface of the Tube; and so choak up and obstruct the passage of the Matter, which otherwise would be emitted very vigorously from thence. And indeed, I have always observ'd, that moisture is a great Enemy to all Experiments of this kind: The reason of which, I think, is pretty obvious from what was just now hinted about the *resistance*.

How-

However, there is an Experiment, which looks, as it were, like an ocular Demonstration of this; in which, the resistance of the Effluvia by certain little Bodies interpos'd, was render'd manifest even to the Sense. For having plac'd a piece of fine *Muslin* between the Tube and the foremention'd pieces of Leaf-Brass; all the excitation I could give the Effluvia by strong rubbing, would not raise them so far, as to make them give the least sensible motion to any one of those little Bodies, even tho' the Tube was held very near them: yet at the same time, if the Muslin were taken out of the way, the Effluvia would impress those Bodies so vigorously, as to give them a motion at 3 or 4 times that distance. And this Phenomenon, I think, will easily be allow'd to be the meer Effect of the Obstruction of the passage of the Effluvia by the fine Threads of the Linnen; which threads either wholly stop'd and detain'd, or else broke and obtunded the force of those active parts emitted from the Tube by Friction; so that they could not shake or agitate the light Bodies, that lay expos'd to them.

I could add moreover, That not only the *moist*, but the *cold* Temper of the Air, may be partly a cause of the Differences discernible in the Effects of this Experiment: For when it was first made, it was Summer-time, as well as dry weather; and the success seem'd to be more considerable then, than it has been since the declension of that Season of the Year. And indeed, considering that all those Effects of the Effluvia upon other Bodies, must depend either upon the *quantity of the subtile matter emitted*, or the *force and velocity of its impulse*, or else upon *both these conjunctly*; 'tis not strange that warm Summer-weather should cause a considerable difference: because the
vigorous

vigorous action of the Solar Rays does then more effectually shake the parts of all Bodies, opens and unlocks their Pores, and so makes way for a freer and more plentiful emission of their Effluvia; and because also, at the same time, the more fine and rarefied state of the ambient Medium (the natural result of a predominant heat) will allow them to expand themselves with more advantage. However, in colder weather, the Effects are very sensible, and great enough (notwithstanding the disadvantage arising from thence) to procure any curious Person's Attention to the Phenomenon.

Thirdly, When the Tube became hottest, by the strongest Attrition; the Force of the Effluvia was rendered manifest to another *Sense* too, namely, that of *feeling*. They did not then only produce all the fore-mention'd Effects in a more remarkable manner, but were also plainly to be *felt* upon the Face, or any other tender part, if the rubb'd Tube were held near it. And they seem'd to make very nearly such sort of strokes upon the Skin, as a number of fine limber Hairs pushing against it might be suppos'd to do. This vigorous Action of the Effluvia put me upon an attempt, to find in what manner such a motion was propagated, and in what Figure or sort of Track it went along. For which end, I held the rubb'd Tube near the Flame of a Candle, Smoke, Steam, Dust, and the surfaces of Liquids; but without any manner of success.

The reason of which, I attribute to the impediments the Effluvia met with from these Bodies the Tube was plac'd near. For the small parts of Dusts and Powders, the steams of Liquids, the oleaginous Fumes of Flame, and the like sort of parts in Smoke it self, immediately adher'd to the surface of the Tube,

- Tube, and so kept in the Effluvia: which therefore requir'd the assistance of a fresh Attrition to open their passage and give them vent again.

And thus much for the First part of this Experiment; in which *the Tube was rubb'd while it continued full of Air.*

Secondly, We are now to consider the Effects of the Attrition of the same Tube, when the Air was exhausted and drawn out of it. And here I observ'd, that tho' it were rubb'd with equal or greater force than was imploy'd about it when full of Air, yet *the attractive power was very little discernible.* The Effluvia scarce discover'd themselves, by any motion or disturbance given to the Leaf-Brass, even tho' it was plac'd within a quarter of the distance at which it had been attracted before. Indeed, when the Tube was very warm, and held very near, there would be some little motion given to those small Bodies; but 'twas too small by far to be compar'd with what was impress'd when the Tube was full of Air. Besides, I doubt not but there was some portion of Air left in the Tube, and so the Attraction might continue in proportion to the quantity of Air remaining: or perhaps, the heat produc'd by a smart Attrition of the Glass, may (in this Experiment, as well as some others) be in the stead, and supply the place, of such a quantity of that Element.

But to return to the Experiment. When the Air was let into the Tube again, the attractive power was immediately restor'd. Before any new Attrition could be given the Tube, or that it was remov'd from the distance and position it was held at when exhausted, even then did several of the fore-mention'd Bodies (which before seem'd to be wholly at rest) begin suddenly to move; and some of them:

them were attracted up to the Tube it self. And then, lastly, upon a fresh Attrition, the Tube recover'd it's *Electrical* quality as vigorously as at first.

Hitherto we have consider'd the Phenomena of the Tube both when full of Air, and when exhausted; but in each case *still in the Light*. We are now to consider what occur'd upon the *like Attrition given it in the Dark*. And first, the Tube being full of Air; 'tis observable, that when the Glass became warm, a *Light would continually follow the motion of the Hand* backward and forward: And at the same time, if another Hand was held near the Tube, a Light would evidently break forth from it, and That *accompanied with a Noise*, resembling that of the cracking of a green Leaf in the Fire; tho' not so loud: Yet when the Experiment has been made in a very still and silent place, I have heard several cracks, at seven or eight foot distance, or more. Farther; if any other Object (besides the Hand) was brought near the Tube, tho' it did not touch it, yet a *Light would fix upon it*, and give much the same appearance as upon the Hand; as I have tried with *Gold, Silver, Brass, Ivory, Wood, &c.*

But now, when the Tube was *exhausted of its Air*, there was a considerable difference, as to this Light and its effects: For upon the first Attrition of the Tube, a *much greater Light indeed did ensue*; but then the *quality of giving Light to a Body held near it, seem'd to be quite lost*: And (which is another Difference no less remarkable,) the Light produc'd upon the Attrition of the exhausted Tube, appear'd to be wholly within it; whereas that which was discover'd when the Tube was full of Air, seem'd to be altogether on its out-side.

And

And thus much for the Experiment, as made upon a Tube or hollow Cylinder of Glass.

Having now, in the next place, procur'd a solid Cylinder of Glass; I made the same Trials with that as the former; and found no great difference in the Effects; only the emission of the Effluvia seem'd to continue a little longer, but not to attract at a greater distance than the other (as far as I can yet discover). With this new solid Tube, I made the following Experiment.

I took a little quantity of Lamp-Black, and having dried it on a Paper before the Fire, I expos'd it to the Tube (which had been rubb'd till it was warm): And it was no small Entertainment, to see how briskly the little black Particles were agitated by the force of the Effluvia from the Glass. They appear'd to rise and fall, to move upwards and downwards with great velocity. And tho' their Specifick Gravity was so small, that when they fell by their own weight, they could not be heard; yet they were return'd upon the Paper with such a force from the Tube, that their strokes upon the same made a very sensible noise. So great was the repellent force of the Effluvia from the solid Tube. So that now laying the accounts of this Experiment together, we have the following Heads of matter of Fact to consider.

The Various surprizing motions of the Leaf-Brass, expos'd to the hollow Tube, upon the Attrition of it.

The Encrease of that Effect, upon the encrease of the Heat in the rubb'd Tube.

The Difference observable in that Effect, according to the different Temper and Constitution of the Air.

The Destruction or Ceasing of that Effect, upon the exhaustion of the Air out of the Cavity of the Tube.

The Return of that Effect, in a good degree, upon the Return of the Air, even without the help of any new Attrition: and the compleat recovery of it to all degrees, when that Attrition was again given.

The Emission of a Light from the Tube full of Air, when rubb'd to a degree of warmth in the dark: and this Light accompanied with a cracking noise.

That Light's fixing and settling it self upon Bodies laid in its way.

The Loss of this Quality (of fixing upon other Bodies,) tho' with the advantage of a bigger Light; when the Attrition was given to a Tube exhausted of its Air.

The different Seat and Place of the Light, when the Tube was full of Air, and when exhausted: being in the former case without, and in the latter wholly within the Tube.

Lastly, The same Effects (except what depended on the Exhaustion) produc'd by a solid, as by a hollow Tube.

Having now deduc'd some things from the Experiment, which may (I think) pass without much dispute for Truths and Matters of Fact; I would propose some things by way of Query, in order to a farther clearing of this Subject.

Query 1. How is it that Attrition does excite and bring forth the Effluvia of Electrical Bodies? They are, in the present case, propagated from the Body of the Glass with a considerable force: What is the immediate cause of that Impetus? And by what means do they come to be so affected?

Query 2.

Query 2. Why are the Effects of the Effluvia so much greater when the hollow Tube is full of Air, than when it is exhausted?

Query 3. Why does the Light produc'd upon the Attrition of the exhausted Tube, appear wholly within it; and That produc'd upon the Attrition of the Tube full of Air, appear altogether on its out-side?

Query 4. Can the preserving, or the taking away the Equilibrium between the external Air and the Air included in the Cavity of the Glass Tube, be of any moment towards the production of the Effects mention'd in the two last Queries?

*A Continuation of the Experiments on the
Attrition of Glass.*

I Procur'd a Glass of a Figure as nearly Cylindrical as might be, whose diameter and length were each about 7 inches. The Axis of this Glass lying parallel to the Horizon, and the contain'd Air being exhausted, it had a Motion given it by a Machine of a new Contrivance. And the Effects of this, with respect to the Light produc'd upon the Attrition of it, were much the same as those in the Experiments formerly mention'd. But when the Air was let in again, and the Motion and Attrition given, as at first, I was surpriz'd with the appearance of a brisk and vigorous Light continued between the point of my Finger and the Glass. It was not

Plate VII

only plainly visible on the Finger; but besides, seem'd as it were to strike with some force upon it, being easily to *be felt by a kind of gentle pressure*, tho' the moving Body was not touch'd with it by near half an inch. This Light seem'd to issue from the Glass with a *considerable noise* (not much unlike that of wheezing, tho' something smarter;) and 'twas easy enough to distinguish it from the noise made by the working of the Engine, which notwithstanding was not a small one. And the Phenomenon was the same, as to both the Parts of it, (I mean both the *Light* and the *Noise*,) when the Experiment was made in the Day-light, as when in the Dark. For in a very light Room, an Hour or two after Noon, the Glass being put into Motion, and the Attrition made, and the Finger approach'd near it (as before;) a pure Purple Light immediately extended it self from the Finger to the Cylinder, and was accompanied with the like noise as before-mention'd. And this Trial I have repeated several times, at different hours; but still with the same Success.

I render'd these luminous Effluvia more remarkably conspicuous, and at the same time more pleasing to the Eye of a Spectator, by another Experiment made after this manner: I took a bit of fine *Muslin*, and sew'd it to a couple of Wires bent circular-wise, that so it might surround the upper surface of the Glass; which it did at near 4 inches distance. The Muslin I made as ragged as I could, by breaking the Threads of it every where. The Glass being then put in motion, and the Friction made (as usually;) the Light threw it self abroad vigorously, and settled in small lucid sparks upon the ends of the torn Threads; looking there like so many little Stars, seen by

a

a good Telescope in the *Milky way*. And from all these little Balls of Light together, there resulted such a sort of a whiteness, as may be observed in the Heavens by the faint and weak Lights of all those small Stars mingled together.

In all these cases, I could never find *that the addition of any external Heat, would do any thing to increase the appearance of the Light produc'd*. I tried by placing a red-hot Iron just under the moving Glass; but this would do nothing at all, without the Attrition of the Glass; and if the Glass were rubb'd, the Effect was no greater, than if the hot Iron had not been there. And the result (as to this matter of *heat*;) was the same, both when the Glass was exhausted of its Air, and when it was full.

And thus much, as to the Light produc'd by the Attrition of the Cylindrical Glass.

Secondly, as to the *Electricity* of it; I did not find this Effect more considerable here as to quantity, than what was related in the former Experiments of the Tubes. But notwithstanding, I have discover'd some Properties of this Electrical matter, which may seem wonderful to those that nicely consider them; *since they afford us a sort of representation of the great Phenomena of the Universe*. For, having observ'd that light Bodies, plac'd near any part of the rubb'd Cylinder, seem'd to be equally attracted; I contriv'd a Semi-circle of Wire, which I could fasten at a constant distance, making it encompass the upper Semi-cylindrical Surface of the Glass, at 4 or 5 inches distance. This Wire had several pieces of Woollen Thread fasten'd to it, so as to hang down from it at pretty nearly equal distances. The length of them was such, that being extended in a direction towards the center of that imaginary
Circle

Circle on the surface of the Glass, in the Plane of which the Wire was plac'd; they would then reach within less than an inch of the Circumference of that Circle: but if left to their own liberty, they hung in that parallel position to each other, which is represented in *Fig. 1.* The Cylinder was plac'd with its Axis parallel to the Horizon; and in this posture, it was turn'd swiftly about; and then by the rapid motion and agitation of the surrounding Air, the Threads were forc'd into such positions, as are express'd in *Fig. 2.* viz. they were lifted up and bent upwards from the Axis of the Cylinder.

All this while, here was only the swift motion of the Cylinder round its Axis, without any Attrition. But now, when I came to apply my Hand to the lower part of this Glass (so swiftly whirl'd about,) and consequently to add *Attrition* to the former motion; *the Threads presently began to change their direction, and all harmoniously pointed to the center of the Circle, in whose Plane the Wire was plac'd.* Neither were they at all disorder'd or flung out of that position, by the Wind occasion'd by that violent motion; but (as if there had been no such hurry of the Air about them) they still persisted in their central direction. And to render it most sensibly convincing, how absolutely this Effect depended upon the Attrition; I found I could by shifting the place of the Attrition hither or thither, draw the Threads towards this or that end of the Cylinder; but yet they all still went uniformly converging towards some center in the Axis of it; so that they form'd themselves into a sort of Conical Surface.

Farther;

Farther; if the Wire with its loose Threads was revers'd, so as to encompass the lower part of the Cylinder, (as before it did the upper part;) yet the Effect still answer'd with the same exactness. For the Threads were all erected into so many strait lines, still directing themselves to a center in the Axis of the Glass. Fig. 4.

Hitherto the Axis of the Cylinder was plac'd *Horizontally*; in the next place, I set it in a *vertical* position, so that it stood perpendicular to the Plane of the Horizon; in which case I made use of a Wire-hoop, which was necessarily to be plac'd parallel to the Horizon, that it might encompass the Cylinder in the same manner as the Semicircular-Wire did before: only one small part of this circular Wire was left open, to make way for the touch of the Hand, which was to give the Attrition. And the Wire being thus plac'd, it was evident that the Threads (without some external force to support 'em) must all flagg and hang perpendicularly downwards. Yet, as soon as the Motion and Attrition were given, the Threads presently began to be extended; and, as if they were become stiff and hard, form'd themselves into an Horizontal Plane; their loose ends pointing to a center in the Axis of the Glass (as before.)

And thus in all sorts of Positions whatsoever, both of the Wire, and of the Glass too; were the Threads acted by a sort of centripetal force, to the Laws of which they were always conformable.

The matters of Fact observable in this Experiment, may be compris'd under these Heads.

The Continuation of a brisk Light between the Hand and the Glass.

The

The *Sensible Force* and *Pressure* of that Luminous matter; which was very easie to be felt.

The *Noise* accompanying the eruption of the Luminous matter, and the Loudness of it, which render'd it distinguishable from that of the Engine.

The *Constancy* of the Light and Noise at all Seasons.

The *Indifferent regard* of this Light to the presence or absence of Heat.

This as to the *Light*.

As to the *Electricity*.

An *Equable diffusion* of *Electrical Matter*, in Planes parallel to the Basis of the Cylinder; and the *Direction* of the Threads to the center of those Circular Planes.

The *steady and unvaried* Direction of the Threads, notwithstanding the Wind occasion'd by the violence of the Motion.

The *Easie Excitation* of the Electrical Matter in any of the parallel Planes of the Cylinder, occasion'd by the shifting of the Hand into a new place: Also the *Variation* of the *Figure*, and *Direction* of the Threads, consequent thereupon.

The *Constant Direction* of the Threads to some center in the Axis, in *all the Positions* of the Wire, and of the Cylindrical Glass; the Effect being still the same, whether the Wire was *above* or *under*, or the Glass plac'd with its Axis either *parallel* or *perpendicular* to the Horizon.

Some

*Some farther Experiments relating to the Electricity
of Glafs.*

IT has been shewn before, that the ends of the Threads which were dispos'd in the *Semicircular Wire*, were, upon the Attrition, directed to a Center in the *Axis of the Cylindrick Glafs*. There are some other Observations and Discoveries which I have since made, relating to the same thing; which are no less surprizing; and which I shall therefore here give in their order.

EXPERIMENT I.

WHEN the Attrition of the Glafs has been continued a little while, and the *Woollen Threads* laid hold of by the *Effluvia*; then, tho' the Glafs had no motion at all, and no Friction continued, yet would all the Threads continue in their *strait directed posture*, and that for the space of 4 or 5 minutes: nay, it would sometimes be longer before they could disengage themselves from the force and action of the *Effluvia*.

And while the *Effluvia* acted with so much vigour, as to sustain the Threads in their *Central Direction*; if a Finger (or any other body) were put near the *Extremities* or *Pointing ends* of the Threads,
I they

they would *avoid and flie* from it, just as a *Magnetick Needle* does, when the *disagreeing Pole* of the Loadstone approaches to it.

But if the Finger were held at about an inch distance from the end of any such Thread, the Thread would (usually, tho' not always,) be *attracted towards it*; plainly removing it self out of its place, to come nearer thereto.

If any Body be *interpos'd between the Glass and the said directed Thread*; then the Thread, depriv'd of the directing force of the Effluvia, immediately returns to its *first and natural position*, viz. such as its own gravity gives it. But if that interpos'd Body be *taken away again*, then (provided the Thread has not remov'd it self too far out of the reach of the Effluvia) it will *return again to its central tendency*, and remain in it, till its gravity becomes too great for the decreasing force of the Effluvia to support it any longer in such a Direction.

Fig. 7. NB. I have since tried the same thing with a
Plate VII Glass Globe, and found that when the Attrition was made, it would attract the surrounding Threads in all manner of Positions, and direct them to its Center. What we may observe from hence therefore, is,

First, The vigorous and lasting Action of the Effluvia, excited by this Attrition; seeing the Tension of the Threads was still continued, tho' the Friction and Motion of the Glass were both ceased.

Secondly, A plain Instance of a Repulsive and Attractive force. For the Threads avoided the Finger, (as if acted by some centrifugal force,) when it approach'd very near their Extremities: and at another di-

distance, something like a *centripetal* force would cause them to incline and *move themselves towards it*. So that in these smaller Orbs of matter, we have some little resemblances of the Grand Phenomena of the Universe.

Thirdly, The certain Dependence of this Phenomenon (*viz. the extension of the Threads*) upon the action of some Matter, whose Direction is in strait lines towards the Glass. For upon the interposition of any Body between the Threads and the Glass, they lose their regular extension, and hang as their own weight causes them. And what can such Interposition of a Body possibly do in this case, but only interrupt the course of the Matter which is the cause of the Extension, and consequently make way for the gravity (which was overcome by a superiour force before) to exert it self again?

EXPERIMENT II.

I Took an *Hemispherical Glass* (such as is represented at Fig. 5.) of about 6 inches diameter: In-^{Plate VII} to this I convey'd a *Stick*, in manner of an *Axis*, which had the *Woollen-Threads* (formerly made use of) tied about it. The Glass was screw'd by the *Neck* to one end of a *Spindle*; and being fix'd on the *Machine*, the Great *Wheel* was turn'd, and^{Plate VII} the Friction made on the outer surface of the Glass, as usual. And now the Threads presented a Phenomenon,

phenomenon not a little pleasant and surprizing to behold; but yet such as I expected and hop'd for, in the contrivance of this particular *Apparatus*. For here was just the *Reverse* of what happen'd when the Semicircular Wire was plac'd on the out-side: That is, the Threads *here* issued like Rays, *from* a Center *outwards*; as *there* they *converg'd* to a Center *within*. The nearer they were to the *concave* surface of the Glass, the farther their Extremities diverged from one another, in *this* case; as in *that* case, the nearer they were to the *convex* surface, the nearer were their Extremities to one another. In either case, the *Divergency* or *Convergency* was *from* or *to* some Center in the Axis; and all the difference was, that in the one case the tendency was *from without, inwards*; and in the other, 'twas *from within, outwards*. The appearance of the Threads in this last Experiment, was as is represen-

Plate VII ted at Fig. 6.

And what was very considerable farther, was, that while the Threads lay in *this* position, like so many Rays of a Circle extended; if the Finger were held near the *out-side* of the Glass, a Motion would be communicated to the point of that Thread which was nearest within: so that by the motion of the Finger the Thread would be driven any way before it. And it would seem to fly and avoid the Finger, held on any side; tho' the convex surface of the Glass were not touch'd by it, by $\frac{1}{2}$ inch and more (as I have sometimes seen.) Likewise, if the Threads were remov'd to the *out-side*, and the Finger mov'd about within, the Threads would play about with the like motion.

And

And indeed, generally speaking, the Threads seem to avoid the approach of the Finger: tho' I have sometimes observ'd them to jump suddenly towards it, at more than an inch distance.

From the Experiment now recited, we may observe,

The remarkable Uniformity and Agreement conspicuous in this Phenomenon, with That where the Threads were dispos'd on the out-side of the Glass. For in both, the Threads were directed, according to the course of the matter; which in one case acted one way, and in the other the contrary. And by comparing both, 'tis very plain, that there is no more than one and the same Cause of both. For the same Cause which in one circumstance (*viz.* when the Threads were plac'd on the out-side) would make them converge towards the Convex surface of the Glass, would also in the contrary circumstance (*viz.* when they were plac'd on the in-side) make them diverge towards the Concave surface.

Again, the Agreement answers, not only with respect to the Direction of the Threads, but also to the Motion of them. For in the former Experiment, the Threads would sometimes avoid, and sometimes incline towards the Finger: and in the Experiment now mention'd, there were the like Indications both of a Centrifugal and Centripetal Force.

EXPERI-

EXPERIMENT III.

I Took a Glass Globe of 9 inches diameter, which being *exhausted* of its Air, I fix'd to a Machine, to give Motion to it, perpendicular to the Horizon. And to another Engine I fix'd another Glass Globe, plac'd at a distance something less than an inch from the former Globe; and *having none of its Air drawn out*. The Machines being set to work, I apply'd my naked Hand to the Globe which *was full of Air*; the Effluvia of which (excited by the Attrition) quickly reach'd the *exhausted* Globe, and *produc'd a Light* on that part of it which was nearest the other. Now here was not the Friction of any other Body upon this Glass, to raise the Effluvia, and produce a Light; there was nothing *but the bare Action of the Effluvia from the other Globe*, which Effluvia supplied the place of a more solid Body, and made such an Attrition as was necessary to the production of the Phenomenon. The Light was pretty vigorous, and spread it self on the Globe as far as the Effluvia were capable of striking on it. Its Colour was not near so much inclined to *Purple*, as it was when the Light was produc'd by the attrition of the Hand: But it would continue upon the Globe for half a minute or more, after the motion of the rubb'd Glass had ceas'd. On the other Hand, if the rubb'd Glass were kept in motion, the other being at rest; the Light would die presently;

sently; but immediately recover'd again, upon the first motion given to it.

As an Appendix to this Experiment, I shall relate another, much to the same purpose.

I took a *long Glass*, whose Air was exhausted, and which had lain by in that state above six Months. After I had rubb'd this Glass a little with my Hand, to clear it of all Moisture on the surface; I held it over the *unexhausted Globe*, which was then in motion; and at the same time also I gave it (*viz.* the *unexhausted Globe*) an Attrition with my Hand; upon which there were immediately large and surprizing *Flashes of Light* produc'd in the long Glass, tho' it neither touch'd the moving Globe, nor was provok'd it self by any immediate *sensible Attrition*.

The Matters of Fact afforded by this whole Experiment, are reducible to the following Heads.

The Production of a continued Light on one Glass exhausted of its Air, by another, at a distance from it, being rubb'd while it was full of Air: both being in motion, but one only rubb'd.

The Continuance of the Light on the exhausted Glass, for some time after the Motion of the other Glass is at an end.

The sudden Ceasing of the Light, when the exhausted Glass is at rest, tho' the full Glass (on which alone the attrition is made) be kept in motion.

The Production of an interrupted Flashing Light upon an exhausted Quiescent Glass, held over another unexhausted one in motion; the unexhausted one being rubb'd at the same time.

And from hence we may observe,

First,

First, *The Force and Vigorous Action of the Effluvia, by which they perform the Office of a solid Body.* In other Experiments, Lights were produc'd by the attrition of *one solid Body against another*: but here a Light is produc'd by *the friction of a very subtile Fluid upon a Solid*. So that this may come in, amongst many others, as an instance of the powerful Effects of *small Bodies*, when put into brisk and vigorous motions.

Secondly, *The great Interest which the motion of the exhausted Glass has in the continuance and preservation of the Light*: For if That be stop'd, the Light dies away presently, tho' the unexhausted Glass be in motion. And indeed 'tis evident, that the Action of the Effluvia upon the said exhausted Glass, is more extensive, (and so capable of producing more considerable Effects,) when that Glass is in motion, and the parts of it consequently successively expos'd, by a quick Revolution, to the strokes of those Effluvia; than when it is at rest, and the Effluvia beat only on one and the same particular part of the Surface.

And this is effectually confirm'd by the Observation of the kind of Light produced on the exhausted quiescent long Glass: For that was not a continued, but a *Flashing Light*; that is, such as disappear'd as soon as it was produc'd on the Surface of that quiescent Glass.

Thirdly, *The difference in Degree and Intensity of the Colour, which the different Circumstances of the Friction are capable of producing.* For the Light produc'd by the *Attrition of the Effluvia* did not come near, as to the degree of Purple, to that Light which was produc'd when the *Attrition was made by the Hand*.

*An Account of an Experiment, confirming the Production
of Light by the Effluvia of one Glass falling on ano-
ther.*

HAVING observ'd that the Effluvia of Glass, when they fell on an exhausted Glass in motion, would exhibite such an appearance, as if it were rubb'd by a visible solid Body; I thought this further Confirmation of it would not be unacceptable. I took a large Receiver, of such a form as is express'd by a a a a; (Fig. 1.) within the Body Plate III of which I fix'd another, of such a form, and in such position, as is represented by b b b b. Their Axes were parallel to the Horizon, and fix'd one within another at c c. The outward Surface of the inward Glass was at least an inch distant from the inward Surface of the outward one: and they were both turn'd by two large Wheels d d d d, whose Bands related to the small Wheels e e e e, fix'd on the Axes of the Glasses. Before the Glasses were thus adapted to each other, the innermost was exhausted of its Air; and then being set as the Figure describes, I order'd that Wheel only to be moved which gave motion to the great Glass. The Thought which guided this Process, was this; that when the Effluvia of the great Glass (by the application of my Hand upon it) should reach the other; this other, notwithstanding it was at rest, would nevertheless be influenc'd by the Effluvia, and give a
K Light.

Light. The Effect answer'd my Expectation; for the Light appear'd, and spread it self in numerous branches all over. This done, I caus'd the other Wheel (*viz.* that which gave motion to the included Glass,) to be turn'd; and then the Light became much more considerable, and, I think, the greatest that has yet been produc'd in any Experiment made on this Subject. And I doubt not, but 'twould have been much more considerable, had the inward Glass fitted so, as nearly to touch the inward surface of the outer Glass; the Effluvia of which (as it appears to me) would then have been capable of acting with more vigour on the inclos'd exhausted moving Receiver.

Having in the next place caus'd both the great Wheels to turn the Glasses one and the same way, with as equal a degree of Velocity as they could; I did not find but the Light was then as strong, as when their motions were just the reverse. So that (as far as I can perceive) neither the contrariety nor agreement of the Motions does contribute any thing to this Phenomenon; but Motion it self (without any particular Rules or Limitations) is absolutely necessary: As this, and the whole Course of Experiments on this Head, abundantly shews.

I observ'd farther, that tho' the Effluvia seem'd to be equally distributed on the outward surface of the inward moving Glass, yet the Light appear'd most vigorously on that side of it next the Attrition. And when either of the Glasses was at rest, the other continuing in motion, (I say *either*; for upon Trial I found very little difference either way;) the appearance of the Light would remain a considerable time within the exhausted Glass, till the Effluvia of the other were no longer capable of acting with

with a force upon it, requisit to produce the Effect. 'Twas not a little surprizing also to observe, that after both Glasses had been in motion for some time, and the Hand apply'd during that time to the surface of the outer one, that then, the Motions both ceasing, and no Light appearing at all, if I did but approach my Hand again near the surface of the outer Glass, there would be Flashes of Light (like Lightning) produc'd in the inward Glass; just as if the Effluvia from the outer Glass, had been push'd with more force upon it by means of the approaching Hand.

The matters of Fact afforded by this Experiment, may be compris'd under the following Heads.

The Production of a Light by the Effluvia of the outer circulating Glass, falling on the inward quiescent one.

The Extraordinary Augmentation of the Light, upon the circular motion of the inner Glass together with the outer one.

The Constant and Unvaried state of the Effect (as to the Vigour and Strength of the Light,) whether the two Glasses were moved the same or the contrary way.

The most vigorous appearance of the Light upon the side next the Attrition, when both the Glasses were mov'd; notwithstanding the Effluvia seem'd equally distributed on the surface of the inward Glass.

The Continuance of the Light in the exhausted inner Glass till the Force of the Effluvia was spent; when either Glass was in motion, and the other at rest.

The Flashes of Light produc'd upon the inner Glass, by the approach of the Hand to the outer one, no Light at all appearing before: both Glasses being then quiescent, tho' both had been in motion, and the outward one rubb'd all the time of the Motion.

Query 1. Why is a Medium so very-much-rarefied, (as that in the exhausted Receiver) requisite to produce a Light?

Query 2. Does not this Light, produc'd by the Attrition of the Effluvia, shew, that Minute and Fluid Bodies, when put in violent motion, are capable of performing the same Effects as others of a more gross and solid Nature?

S E C T.

S E C T. III.

An Experiment shewing the Difficulty of separating two Hemispheres, upon the injecting an Atmosphere of Air on their outward Surfaces, without exhausting the included Air.

TH E best Proof that can be given of the Truth of any Hypothesis, is, that the Experiments made for that end, do all of them, and every way agree: That trying Nature on one side, and on the other, yet every way she still confesses the same thing. Thus with respect to the nature of Sounds; 'tis demonstrable that the Air is the proper Vehicle or Medium for the propagation of them; because Sounds do not only *lessen and grow weaker*, according to the degrees of the Air's *Rarefaction*; but also become more *intense and strong*, according to the degrees of its *Condensation*. And I offer the following Experiment, to shew, that we have the very same degree of certainty of the Pressure and Gravitation of the Air; in that the very same Effect is produced, when we make use of a *condens'd Atmosphere* to work against *common Air*, as when we make use of *common Air* to work against a very-much-rarefied Medium or *Vacuity*. This Experiment, I hope, will be no less than decisive of the Point so long in agitation, and set the Truth free even from
any

any possibility of being attack'd by the Objections of the Favourers of *Suction* and the *Funicular Hypothesis*. 'Tis true, the Doctrine of the Air's Pressure has been fairly and clearly demonstrated by a great number of Experiments, already made for that purpose. But still these People have found some Shifts and Subterfuges, by which they have made a shew of evading the Conclusive Force of the Experiments. They have still had room left to say something or other, which, how little soever it has really been to the purpose, has yet serv'd to keep the Controversie alive, and make the Unskilful or Unthinking believe they had some Probability on their side. Now, to shew how unreasonable those Evasions hitherto made use of, have been; as also at once to set the matter in a satisfactory Light; I proceeded in the manner following.

See Plate
IV.

I took a strong Glass *Receiver*, open, and arm'd with *Brass Hoops* at top and bottom; in which I plac'd two *Brass Hemispheres*, joyn'd together on a wet Leather at b b b b (the diameter being $3\frac{1}{2}$ inches), as also a *Mercurial Gage* represented by c c c c. To the *Brass Hoops* were applied two *Brass Plates* d d d d, with wet Leathers between them. To the upper Hemisphere was screw'd a large *Brass Wire* e e, that pass'd through a *Box* of Leathers f f, which was screw'd on the upper Plate; and this Wire could easily be mov'd up and down without suffering any Air to pass in along with it. This moveable Wire had a *Cock* g g g g screw'd at the upper part of it, thro' which the Air was to be injected. In this manner were the upper and lower Plates firmly screw'd to the Receiver by the Frame and Pillars h h h h h h.

These

These things thus provided; an Atmosphere of Air was thrown into the Receiver; the quantity of which injection was easily discover'd by the fore-mention'd Gage c c c c; the Air therein possessing but half the space it did before. When this was done, the Syringe i i was taken off, and an Iron with an Eye, represented by k k, was screw'd on in its place; by which means the whole Apparatus was suspended on a Triangle l l l l l. (Note, that the moveable Wire and upper Hemisphere related to this Iron; all the rest being part of the weight made use of to separate them.) After this, the Scale which hung at the Bottom had so much weight put into it, that all together made full 140 pound; and nothing less than this weight of 140 pound, would part the Hemispheres; so powerful was the Force and Pressure of the Atmosphere injected on their outward Surfaces, to hinder that Separation and keep them together. Now how those that espouse the *Funicular* Hypothesis, or that of *Suction*, will solve this from their Principles, I can't imagine! For how is it possible that any thing of that kind should take place in the matter before us? How and which way does any *Suction* drive these two Hemispheres together with such a force? or where's any room for a *Funiculus*, that may be imagin'd to be the cause of their Union and Compression? I can't see but the Matter of Fact is plainly beyond all Exceptions; and that What I have propos'd, is no less than an *Experimentum crucis*. Therefore,

Corollary. From hence the Doctrine of the Pressure of the Air is certain.

For there can't possibly be any thing assign'd as the Cause of this Compression of the Hemispheres, but the Pressure of the external condens'd Air on their Surfaces. For, whatever other Cause any one shall think fit to assign; it may easily be shewn to be impossible, from the Circumstances of this Experiment. From whence the certainty of the Pressure of the Air being establish'd, I may fairly conclude, that the same Principle was the Cause of the Compression of the Hemispheres in the common *Magdeburg* Experiment also. For Nature would not do it by *Pressure* here, and by *Suction* there: This would be such an unequal acting, that the Wisdom and Simplicity of Nature is by no means to be blemish'd with such an Imputation.

And therefore I think I may venture to affirm, That all the Objections that have been made against this Doctrine, have (at best) been the Result of nothing else, but fallacious and mistaken Reasonings.

However, to prevent all Scruples in Them that may be apt to retain any Doubt of the Air's Pressure, I shall add one or two convincing Circumstances more.

First, Having caus'd the same two Hemispheres to be exhausted of their Air, and none but the common open Air being about their outward Surfaces; I found that the same weight was requir'd to separate them then, as was requir'd for their separation when they were full of common Air and had an Atmosphere of Air condens'd on their outward Superficies.

Secondly, Having exhausted the included Air, I injected an Atmosphere (the same quantity as in the former Experiment) on the external Surfaces of the He-

Hemispheres; and then I found that 280 pound (which was double the weight before requir'd) was not sufficient to separate them: I was unwilling to add more weight (tho' I knew a small addition must have done it) for fear of breaking some of the weaker parts of the Machine, which might have been in danger by the fall of such a weight; the Experiment being full and conclusive without it.

Now, what can be a plainer Demonstration (even to Sense,) of the Pressure of the Air, than this? Here was no greater quantity of Air injected on the out-side of the Hemispheres, than when the Common Air was left within them; and yet they were press'd together by a force above twice as great as that in the former case: (for the force that compresses, is always proportionable to the weight requisite to make the separation.) Therefore that same Air, contiguous to their outward Surfaces, *press'd* against those outward Surfaces; and that with a force above twice as great as it did in the former case. This Property of the Air therefore, is certain beyond all dispute.

L

S E C T.

S E C T. IV.

An Experiment concerning the Proportion of the weight of Air, to the weight of an equal bulk of Water, without knowing the absolute Quantity of either.

I Took a Bottle which held more than 3 Gallons, (but how much more, we have no occasion at present to take notice of,) and of a form something oval: which Figure I made choice of, for the advantage of its more easie Libration in Water. Into this Bottle I put as much Lead as would serve to sink it below the Surface of the Water. And the reason why I chose rather to have the weight of Lead inclos'd within the Bottle, than fix'd any where on the out-side, was, to prevent the Inconveniencies which in the latter case must needs have arose from *Bubbles of Air*: For these *Bubbles* would have inevitably adher'd to, and lurk'd in great plenty about the body of the Weight, had it been plac'd on the out-side: Which must have caused some Errors in the Computations of an Experiment that requir'd so much exactness and nicety.

These things thus provided; the Bottle (containing Common Air so clos'd up,) was by a Wire suspended in the Water, at one end of a very good Balance; and was counterpoiz'd in the Water by a weight
of

of $358\frac{1}{2}$ grains in the opposite Scale. Then being taken out of the Water and screw'd to the Pump, in 5 minutes time it was pretty well exhausted; the Mercury in the Gage standing at near $29\frac{1}{2}$ inches. After which (having turn'd a Cock that screw'd both to the Bottle and the Pump, and so prevented the Air's return into it again,) it was taken off from the Pump, and suspended as before, at one end of the Balance in the Water. And now the weight of it was but $175\frac{1}{2}$ grains; which therefore subtracted from $358\frac{1}{2}$ grains (*the weight of the Bottle with the inclos'd Air, before it had been applied to the Air Pump,*) gave for the difference 183 grains; which difference must consequently be the weight of the quantity of Air, drawn from the Bottle by the Pump.

Having thus determin'd the weight of the exhausted Air, a Cock was open'd under Water, upon which the Water was at first impell'd with a considerable violence into the Bottle, (tho' this force abated gradually afterwards;) and continued to rush in, till such a quantity was enter'd, as was equal to the bulk of the Air withdrawn. And then the Bottle being examin'd by the Balance again, was found to weigh 162132 grains: From which subtracting $175\frac{1}{2}$ grains, (*the weight of the Bottle with the small remainder of included Air, after it was taken from the Air-Pump,*) there remains $161956\frac{1}{2}$ grains, for the weight of a masse of Water equal in bulk to the quantity of Air exhausted. So that the proportion of the weights of two equal bulks of Air and Water, is as 183 to $161956\frac{1}{2}$; which is as 1 to $885\frac{1}{12}$; or, in round Numbers, as 1 to 885 .

And there are two things particularly observable in this Experiment.

First, That in making it after this manner, one need not be very solicitous about a nice and accurate Exhaustion of the Receiver. The Success of the Experiment does not at all depend upon it; for to what degree soever the Exhaustion be made, it must still answer in proportion to the quantity taken out. Neither can any more Water possibly enter into the Receiver, than what will just supply the place and fill up the room, deserted by the exhausted Air.

But, *Secondly*, The Season of the Year is to be consider'd in making of this Experiment. I made it in the warm Month of *May*; the Mercury in the Barometer standing at the same time at $29\frac{7}{8}$ inches.

From whence 'tis reasonable to conclude that a sensible Difference would arise, were it to be tried in the Months of *December* or *January*, when the State and Constitution of the Air, is usually different from what 'tis in the foremention'd Month.

S E C T.

S E C T. V.

An Experiment shewing that the Ascent of Liquids in small Tubes open at both ends, is the same in Vacuo as in the open Air.

I Took three *small Tubes* of different diameters, and fix'd them in a piece of Cork, in an exact *perpendicular* position: Also their lower Orifices were set as nicely in one and the same *Horizontal Plane* as I could. This Cork I fasten'd to a *Wire*, which pass'd thro' some *Collars* of *Leather*, included in a *Box* on the upper Plate of the Receiver; by which means I could at pleasure elevate or depress the small Tubes, without any danger of the Air's getting in. Then some Water, which was tinged *See Fig. 3*
Plate II. with a deep Colour, being set on the lower Plate; the small Tubes (which had never been wetted) were drawn to the upper part of the Receiver by the help of the foremention'd Wire. And the Air being exhausted, the said Tubes were made to descend (by the same Wire which drew them up,) till their lower Orifices were *immers'd* just below the *surface* of the tinged Liquid. This was no sooner done, but the Liquor mounted up in each of them to a considerable height above its surface in the *Bason*; but higher in the *smaller Tubes* than in the larger ones. And, what was farther worth notice, the Liquid

so.

so elevated was also retain'd in these small Tubes, tho' their lower Orifices were lifted out of the Water.

Upon the re-admission of the Air, the Fluid stood at the very same elevation, in each of the Tubes, as it did before. What height soever it mounted to in *Vacuo*; it preserv'd the same, without the least sensible alteration, when the Air was permitted to have free access to it again.

So that the Matter of Fact observable in this Experiment, is contain'd under these two Heads.

First, That the Fluid rose in the small Tubes in the Exhausted Receiver.

Secondly, That the admission of the Air made no change in the Height.

From both which put together, it follows directly (as I take it) that *the Air is not the Cause of the Rise of Liquids in small Tubes*. For if it be, how then does the Liquid come to rise in the exhausted Receiver?

If it be said, that the *Vacuum* is not a perfect one, and there is some portion of Air left in the Receiver; I enquire then, if that small portion of very-much-weaken'd Air left in the Receiver was sufficient to raise the Fluid to such a height, would not a new force of Air let in, have made an alteration, and carried it yet to a greater height? If the Liquid rises by means of that Air left in the Receiver, 'tis certainly by vertue of its Pressure on the Surface of the Stagnant Fluid, into which the Orifice of the small Tube is put: And therefore when that Pressure is strengthened by the force of a new quantity of Air admitted in, this more powerful

ful Cause should produce a greater Effect; and the Fluid should rise higher: Which yet it does not, but keeps at the same unvaried height. From whence, I think, I may without scruple conclude, that the Air has nothing at all to do in this matter: For 'tis plain Fact, that the absence of it does not hinder, nor its presence help the Effect: And what neither helps, nor hinders, no Philosophy in the World will allow to be a Cause.

Besides, if to the Matter of Fact afforded by this Experiment we add a Consideration or two more, it will render the Argument yet more sensibly convincing. For Liquids will rise in small Tubes, *in the open Air* (as we see every day:). Again, they will keep the same height they have risen to in *Pleno*, after the Air be drawn out and they be left in *Vacuo*. Now joyn these two Considerations with the former, and I think it renders the Evidence as complete as can be desired. For if Fluids will rise (in small Tubes) *in the open Air*, and also in the empty Receiver; And if they will keep their height they rose to in *Pleno*, tho' you make a *Vacuum*; and keep their height they rose to in *Vacuo*, tho' you make a *Plenum*; then 'tis manifest, that this Phenomenon is absolutely indifferent, with respect either to the *Presence* and *Action*, or to the *Absence* and *Non-action* of the Air; and therefore that the Air it self cannot be the Cause of it.

I would farther add here an Observation or two that I have made, concerning the Properties of these small Tubes.

First,

First, If a small Tube be bent into the form of a Syphon, then observe how high the Liquid would of it self rise in the shorter Leg of such a Syphon, if it were immers'd in Water; for the Orifice of that shorter Leg of such a Tube, must always be at least as far below the surface of the stagnant Fluid as that Height amounts to, before it will run out at the longer Leg. Which is a pretty remarkable difference between these small and the vulgar larger Syphons. For in Those we are not limited to any certain and particular depth, at which the Orifice of the shorter Leg must be plac'd before the Water will run out at the longer: But in Syphons made of very minute Tubes, such as Liquids will spontaneously elevate themselves in, there is requir'd a certain depth at least, for the immersion of the Orifice of the shorter Leg; since all Depths less than the Height of the spontaneous Ascent, will cause no effusion of the Liquid out of the Orifice of the longer Leg.

And from hence 'tis an obvious Corollary, That in small Syphons, whose Orifices are of different diameters, those need to be plunged to the least depth (for causing the Water to run out at the other Leg,) whose Orifices are the largest. For in Tubes of the largest Orifices, the Fluid ascends of it self to the least height. Wherefore since in order to the running of the Liquor, the depth of the Immersion must be (at least) equal to the height of the spontaneous Ascent; it apparently follows, that Syphons of a larger Orifice will run at a less depth of the shorter Leg's immersion below the Surface of the stagnant Liquid, than those of a narrower Orifice will do.

Secondly,

Secondly, By Trials made with Tubes of various sizes and proportions, I found this to be a constant and perpetual Rule; *viz.* That so much of the Liquor would always remain suspended in them, when lifted up out of the stagnant Fluid; as would be elevated above the Surface of it, while they were immers'd in it. From whence it follows, that some Cause (whatsoever it be) which concurs to the elevating of the Fluid into the Tube, while it is immers'd; does contribute as powerfully to keep it at the same height, after the Tube is taken out of the stagnant Liquid.

An Account of an Experiment, concerning the quantity of Air produced from a certain quantity of Gun-powder fired in Common Air.

I Took a fine Glass Tube, about 36 inches long, the diameter of whose Bore was near $\frac{1}{4}$ of an inch. The upper Orifice had a *Ferrel*, solder'd to a Screw cemented on it, to which was screw'd a Cock: The lower Orifice was quite naked and open, it being no way needful to have any guard set on that part. Near the upper part of this Tube, in the in-side, was fix'd a piece of Cork, notch'd on its edges, to give the greater scope and liberty to the Explosion. The Cork had a small Cavity in the middle of it, the better to hold the Gun-powder, which was let down upon it thro' a small Glass Funnel, before

M

fore the Cock was screw'd on. And in this manner, was the lower Orifice of the Tube plunged under the surface of the Water contain'd in a Vessel. The Cock being then screw'd on, and open; 'twas an easie matter, by sucking at it with ones Mouth, to remove the Pressure of the *inward* Air: by which means the Pressure of the *outward* Air would raise the Water in it to any determinate height. And the Tube being accurately graduated by a *File* on its out-side, one might measure the quantity of the Ascent with all the ease and exactness imaginable. When the Water had got up to the intended mark, the Cock was turn'd, which kept it suspended there. And a *Burning Glass* being applied, the Rays, were drawn to a *Focus* upon the Gun-powder; which fired it very quickly, and forc'd the Water down with a great violence; but it rose again suddenly afterwards; however, it rested so far below the mark it stood at before the Explosion, as was equal to the quantity of *seeming Air* produc'd thereby. The quantity of Gun-powder used in this Experiment, was exactly one grain. And I found that the quantity of space the Water had deserted just after the Explosion, was such as would contain nearly a cubical inch of Gun-powder, the weight of which was equal to 222 grains. So that 222 grains of the same Powder, seem (as soon as fired) to produce something, which possesses the space of so many cubical inches of Air. Now whether the space deserted by the Water is possess'd by a Body of the same weight and density, or which has the same qualities with Common Air, I dare not determine; since an Experiment I lately made (to try how much the heat produc'd by the Explosion of the Gun-powder, might contribute to the largeness of the space

space deserted by the Water) seems to conclude otherwise. That matter was thus:

The whole space deserted by the Water, was divided (length-ways) into 20 equal parts. Now an hour after the firing of the Gun-powder, the Water had ascended about $\frac{1}{10}$ of the whole space, which was 2 inches accurately speaking, or suppose it to be $2\frac{1}{2}$ inches. At the distance of two Hours after the firing, it had got up to near $\frac{4}{10}$ of the same. And then I judg'd it might have been of an equal Temper with the external Air, (and consequently not have given way to the Liquid to have risen any higher.) But continuing the Experiment still farther, I found (to my great surprize) that two hours after the last Observation, the Water had mounted to about $\frac{5}{10}$ of the space. Next Morning (which was about 18 hours distance) it had reach'd near $\frac{7}{10}$, or $\frac{1}{2}$ the first deserted space. And continuing thus to rise, I found that at the end of 12 days, the Water had ascended to something more than $\frac{17}{20}$. And at 18 days, it had reach'd 19 of the 20 parts at first deserted by it. And at this station it rested, continuing there for 8 days, without alteration.

I would observe one or two things here, before I make any *Deductions* from this Experiment.

First, That I all-along consider'd the Temperature of the Air, and found that it contributed nothing at all to this odd Phenomenon.

Secondly, That tho' the Account here given may seem to thwart some Accounts formerly given about the firing of Gun-powder in Vacuo, yet, considering the

vast difference of the Mediums in which the Experiments were made, they may be reconcil'd to one another. For when the Gun-powder was fired in so thin a Medium as *a near Approach to a Vacuum*, 'tis plain that the Air remaining in the Receiver could suffer no more by the Explosion, than in proportion to its quantity: which quantity being so very inconsiderable, the Effects could but answer accordingly. Besides, were those Experiments to be repeated again, some Occurrences, which at the first Trials might pass unheeded, would perhaps be taken notice of, which might render all more easie and agreeable than now it seems to be.

Corol. 1. 'Tis plain, that the matter produc'd by the Explosion, (whatever it were,) was of a springy contractile Nature, and but very little in quantity, in proportion to the space which it at first forc'd the Water out of. For it reduced it self at last into the 20th part of the space deserted by the Water; that is, into the 20th part of a space equal in content to a cubical inch, or 222 grains of Gun-powder. So that it was in bulk equal to no more than about 11 grains, which is nearly the 23th part of the aforesaid number.

Corol. 2. The Contraction or Restitution of this springy matter was not equable and uniform, nor indeed (as far as I could find,) according to any regular Law; but very disproportional with respect to the Times. For the degrees of the Contraction would be as the Spaces (reciprocally) into which the Matter was reduc'd by that Contraction; and the Spaces into which the Matter was reduc'd, were exactly discover'd by the ascent of the Water. Now at one hour (after the firing of the Powder) the Water had ascended 2 of the Divisions;

visions; at 2 Hours, 4; at 4 Hours, 5; at 18 Hours, 10; at 270 Hours, 17; at 432 Hours, 19; where it stood without alteration for the space of 8 Days: So that the encrease of the Waters Ascent, and consequently the Restitution of this contractile Matter, was very far from being equable and regular: At first it answer'd in proportion to the Times; but afterwards varied enormously from that Law; as is apparent. For in the first 4 Hours, it rose 5 Divisions; and at the end of 18 Hours, it had risen but 10: So that in the last 14 Hours of the 18, it had gain'd no more space than it had the first 4 Hours; which was 5 Divisions. And so in the rest that follow, the difference was still greater.

NB. Whether the Matter, which was the Cause of this Phenomenon, were real Common Air or no, is what I will not venture to determine: 'Tis sufficient for my purpose to have propos'd the Matter of Fact, and to have prov'd that this Matter had such and such Properties. To me it seems highly probable that it should be an *Heterogeneous Compound of Common Air, of some Aerial parts residing in the Gunpowder, and of the Nitrous and Sulphureous Matter* which are Ingredients in the same. For all these must needs (I should think) be violently hurried and mix'd together, upon the Explosion; and consequently, must all joyntly compose one Heterogeneous Medium, which afterwards display'd it self by such Effects as I have now mentioned.

The *Elastick or Self-restoring Property* of this Matter, seems chiefly to be owing to the *Air* contain'd therein. And that the *Springs of Air* may be so disordered by a violent impulse, as to require Time to recover their Natural state again, will be very clearly made out by the following Experiment.

An

*An Experiment about disturbing the Spring of
the Air.*

I Took my *Condensing Engine*, and put about $\frac{1}{2}$ a pint of Water into the bottom part of its *Brass Receiver*. Then the upper part being strongly screw'd on, I threw into it with the *Syringe* about 3 or 4 Atmospheres of Air, as near as I could guess: and in this state I suffer'd it to remain something more than an Hour. Then letting out as much of the Air (by taking off the *Syringe*) as would readily go away, I presently screw'd on in its room a *Box of Leather Collars*, thro' which there pass'd a small *Glass Tube*, open at both ends, whose lower Orifice was plunged under the surface of the included Water. After this, in a very little time, I found the Water had ascended near a Foot in the Tube; and it continued rising, till it had reach'd near 16 inches.

Upon a *Repetition* of this Experiment, I let the Air remain in that state of Compression for about 18 Hours. And then (proceeding in all respects as before) I found the included Water ascend gradually in the Tube; and observ'd That motion for the space of 6 Hours: At which time the little Tube was accidentally broken, and so farther Observations for that time prevented. But however, from hence we may infer,

Corol.

Corol. 1. That the Springs of Air may be so disturb'd by violent Impulses, or strong Compressions, as to require considerable time to recover their Natural Tone and Temper again.

Corol. 2. And the Times which the Springs of Air will require for their compleat restitution, will be greater or less, according as the Forces by which they are thus impell'd or compress'd, are greater or less; or according as the Times during which they continue in that violent state, are greater or less. That it should be so, is perfectly consonant to the Nature and Properties of the Air; and that it is so, the Circumstances of the Experiment evince. For when the Air had lain compress'd for about 18 Hours, the Ascent of the Water was more slow and deliberate; it creeping up gradually for the space of 6 Hours together. But when it had suffer'd the Compression but for the space of an Hour; the Water advanc'd upwards in the Tube so fast, that in a very small time it had mounted a whole Foot. And there's no reason at all to doubt, but that longer Time, and more valid Compressions, would produce still greater and more considerable Effects, as to the times of the Springs recovering themselves.

And hence therefore; were this Proportion once settled and establish'd by a sufficient number of Experiments; from the Air's foregoing Compression, one might limit and foretell the Motions of the included Liquid; and, vice versa, from the Motion of the Liquid, one might infer the Air's foregoing Compression.

Corol. 3. Wherever therefore (in any Bodies whatsoever) the included Springs of Air suffer any such Compressions

pressions as these are, or any answerable to these; and there be any contiguous Fluid Matter for these Springs, as they restore themselves, to press upon; these same Effects must follow: That is, the Fluids must be put in motion, and advance according as the others press them; and if they were in any sort of motion before, that motion must be alter'd, and either accelerated or retarded, according as the Course and Direction of the Fluid be either with or against that of the Aerial Springs, while they are in this action of Self-restitution. And perhaps several Phenomena, as well in Plants and Animal Bodies, as in other Systems of Matter in our Globe, may owe their true Rise to some such Cause as this. 'Tis certainly a possible Cause, from the Laws of Staticks; and perhaps inquisitive Persons may find out some Effects of it.

An Account of an Experiment, shewing the Cause of the Descent of the Mercury in the Barometer, in a Storm.

TWAS observable in the late violent Storm of Wind, that the Mercury in the Barometer did not only subside very considerably, but also that, upon extraordinary Gusts, there were sensible and manifest Vibrations of it in the Tube.

Now, to account both for the *Depressions* and *Vibrations* of the Mercury in these and such-like Cases, I contriv'd the following

EXPE-

EXPERIMENT.

Having provided a *Receiver*, A, which held about 16 Quarts, I compress'd in it about three or four times its natural content of Air, by the help of the *Syringe* B: Which Instrument is for that purpose screw'd on at c c. This done, and the *Stopcock* D, fitted to this Receiver, being turn'd; the Syringe was taken off, and a *Brass Pipe* about half an inch diameter, represented by E E, screw'd on in its room. This Pipe is inserted into a well-fitted *Brass Socket*, which is fix'd in a square piece of Wood F F, and that directly against a *Tube*, G G, which enters the same piece of Wood, and is plac'd parallel to the Horizon. Now, out of the same piece of Wood, there rises a naked *Barometer*, H H, the *Cistern* of which lies open to the passage leading from the foresaid Pipe E, to the *Horizontal Tube* G. Besides this, out of that same piece F, there proceeds another *Horizontal Pipe* I I, which runs to another square piece of Wood K K, plac'd at the distance of 3 foot from the former. And this second piece K, has likewise a *Barometer* arising out of it, L L; the *Cistern* of which is also open to the *Horizontal Tube* I, and by that means maintains a *Communication* with the open *Cistern* of the other *Barometer*. All the parts of the Machine being thus dispos'd, the *Stop-cock* was turn'd; and the condens'd Air rush'd out of the Receiver with a great force thro' the Pipe E, which discharg'd it into the *Horizontal Tube* G. The result of which was, that this rapid current of Air so lessen'd the Pressure of the Atmosphere upon the stagnant Mercury in the *Cisterns* of the respective *Barometers*, that the Mercury was made thereby to descend at least 2 inches.

N

Nay,

Nay, that Barometer L L, which was 3 foot distant from the Aerial Stream, *was equally affected as the nearer one H H*; the Mercury subsiding nearly to a just Parallelism in both. And 'tis farther observable, that *as the Force of the current of Air diminish'd, so the Weight of the Atmosphere recover'd its Strength again, and forc'd the Mercury in the Barometers to a gradual ascent.* Hence,

Corol. 1: *We have a clear and natural account of the Descent and Vibrations of the Mercury, in violent Storms and Hurricanes.* For the mighty Force of those gusts of Wind, will weaken the Pressure of the incumbent Atmospheric Columns; from whence a Descent of the Mercury must necessarily follow. And the interrupted uneven Action of those Blasts, or the quick and sudden Returns of them, are capable of producing and continuing the Vibratory Motions (i. e. the quick Ascents and Descents) of the same.

Corol. 2. *Not only the different Forces, but also the different Directions of Winds, are capable of producing a Difference in the subsiding of the Mercury.* That Winds of different Strengths should produce proportional Effects, in breaking the Pressure of the Atmospheric Columns; is as reasonable, as that a greater Power should sustain a greater Pondus, or take off more of the the Pressure of the same Pondus, than a less can do. And 'tis no less evident, that the different Directions of the Aerial Currents, must be attended with different Effects too; those whose Course is from the lower towards the higher Regions of the Atmosphere, having both shorter and finer Columns of Air to encounter the Force of, than those whose Course is from the higher to the lower, where the Columns have both more length and density too. I speak this
with.

with respect to the real difference of the Effect in it self, and not as to the outward sensibleness of it to us; for changes may happen, when we can't or don't observe them. But all other Circumstances being alike, this Circumstance of different *Direction* must, I doubt not, produce a real Variety. And if all other Circumstances are not alike, then the Proportions of the Effects will be had from the Composition of the Proportions (either *direct*, *reciprocal*, or *both together*) of the Causes which make those different Circumstances.

Corol. 3. *Strong Winds may affect the Animal Oeconomy, upon this very account, of their altering the Pressure of the Atmosphere.*

Let us imagine a number of Pipes or Canals, of an *elastick flexible* nature, replete with some Fluid; the Pressure of the incumbent Atmosphere is in this case to be consider'd as a *Pondus*, acting against the Force of these Elastick Canals, with that of their contain'd Fluid. And according to *Mechanick* Laws, these distractile Tubes will be so far compress'd by that incumbent Weight, till a just Equilibrium is produc'd between the two Antagonist Forces; and then they will preserve themselves on both sides in that state, till some farther alteration shall happen, to lessen the *Momentum* either of one or the other. If therefore the Pressure of the incumbent Columns of Air be in any measure broken or taken off; the Canals will restore themselves so far forth by their Elasticity, till the *Momentum* of their Renitency becomes equal to that of the diminish'd Pressure. From whence 'tis manifest, (supposing the contain'd Fluid to be in Motion) that the Rate of the Progress of that Fluid, must needs undergo an alteration, in proportion to that of the Change made in

the external Pressure. For the stronger Pressure will straiten the Canals, and consequently encrease the Velocity of the Fluid: as, on the other hand, the more feeble Pressure will give way to the Canals endeavouring to enlarge themselves; and by that means will contribute to the more flow and deliberate motion of the Fluid. The application of all which to the Bodies of Animals, is very obvious and easie: For they are nothing more than so many complications of branching Canals, and tender flexible Membranes, easily yielding to an external Pressure or Pulsion, and capable of restoring themselves by their innate Spring. The great weight of the Atmosphere is always pressing down upon these Machines; and 'tis the spring and renitency of their parts, which is the counter-balance to it, and preserves them from receiving injuries by it. The Vessels consequently, which serve for the Distribution of the Animal Fluids, being differently straiten'd and compress'd by the various weights of the incumbent Atmosphere; the Liquids are affected with new and different degrees of velocity. And therefore when any extraordinary Changes happen in the Weight and Pressure of the Atmosphere, there must be (*ceteris paribus*) as considerable Changes in the motions of the Fluids. But violent gusts of Wind, Hurricanes, and the like, will necessarily produce very great differences in the weight of the incumbent Atmosphere: And therefore, I say, very considerable Alterations may be made in the Motions of the Liquids in Animal Bodies, by such Causes as these.

From whence it follows, that whatever Changes are possible to be produc'd in Animal Bodies, by the meer alteration of the velocity of the Liquids; are (in some measure at least) producible by very strong

strong and violent Winds : And these changes in the Animal Oeconomy, (*viz.* that depend upon the alter'd Velocities of the Fluids) are not a few.

Corol. 4. The weight of the Atmosphere being diminish'd in one place, it is also as much diminish'd at the same time in another place, which holds a communication with the former.

This is plainly visible in the Experiment it self. For the Force of the Air in the Cistern in the Inclosure F, being broken by the violent eruption out of the Pipe E ; that also of the Cistern in the inclosure K, which communicated with the former, was so far diminish'd too, that the Mercury (whose height depended upon it) subsided in that Barometer just (or near) as much as in the other. And the like Effects must be produc'd elsewhere, when the Circumstances answer to these here.

An Account of some Experiments made on the Phosphorus in Vacuo.

EXPERIMENT I.

HAVING provided a dark Room, I drew some Lines with the *Phosphorus* on a piece of *Blue Paper* : This immediately became luminous in the open Air, and appear'd with a Wave-like Undulating Motion. But being plac'd in a *Receiver*, after some few Exsuctions, the undulation ceas'd ; but the Light seem'd considerably augmented. The *Receiver* being farther exhausted, it grew still brighter, and

and continued with that encrease of Lustre, till an admission of Air, which did sensibly diminish it. This decay of the Light was also gradual, answering to the gradual admission of the Air : Tho' upon the Repetition of the Experiment, some Persons present believ'd the Light not altogether so brisk and vivid as at first.

EXPERIMENT II.

I Took two or three small pieces of *Phosphorus*; which being put into a Glass Dish, I mix'd with it a small quantity of *Oil of Vitriol*, *Oil of Tartar per Deliquium*, and *Oil of Cloves*. This mixture fir'd the *Phosphorus* in the open Air: but it was extinguish'd again by the addition of a little common Water. This Preparation being included in a Receiver; very little Light appear'd. But the Air being exhausted, it became very apparent, with a brisk and vigorous emission of Steams. The Ingredients of this Composition in the Dish, seem'd at the same time to resemble a boiling Flame, and exhibited a copious Light; so that several Objects that were near, became very distinguishable. And this lucid appearance continued till the Air was admitted: but upon that, all became opaque and dark. Neither would shaking the Engine (by which means the mixture it self underwent an agitation) produce any sensible recovery of the Light.

EXPE-

EXPERIMENT III.

HAVING put a small quantity of the foremention'd Composition into a Bottle with a narrow Neck, I included in it in a Receiver; and it yielded then but very little Light. But upon the exhaustion of the Air, it began to be luminous; and the Light improv'd in proportion to the encreasing Rarefaction of the Air, issuing out of the Bottle in a Pyramidical form.

At last (tho' the Receiver was well exhausted) the Steams, then emitted, did fairly ascend in that very rare and thin Medium, and reach'd the upper parts of the Receiver, (which was not a tall one,) but descended down again by the sides of it. Upon the Re-admission of the Air, the Light perfectly vanish'd; and it would have been in vain (as I have often try'd) to have expected the recovery of it in the open Air.

These three Experiments do all exactly agree, in confirming this Conclusion, *viz.* *That the Phosphorus-Light is improv'd, by the Rarefaction of the Air.* Common Air is therefore some way or other an Impediment to the Action of those Steams on which the *Light-giving* Quality depends. It remains therefore to be enquir'd, By vertue of what Property of the Air it is, that the Action of the Luminous Steams is thus impeded?

And I think it highly reasonable to conclude, that *the Pressure of the Atmosphere is that Impediment upon the Luminous Matter in this case.* For the Air, as a *Pondus* incumbent on the Body which contains the

the luminous Matter, urging the Surface on all sides, and rendring it consequently more close and compact; the lucid Steams, whose efforts and endeavours to expand themselves cannot balance this over-ruling Pressure, are by that means kept in, and cannot be discharg'd.

Yet I do not see that we can argue here from the Density and Gravity of the Air, consider'd as a *Medium*. For if the luminous Matter were specifically *heavier* than common Air, it would much more be heavier than rarefied Air in an Approach to a *Vacuum*; and consequently the Steams could not rise, nor the Light appear, (much less improve,) in the exhausted Receiver; as we see it does.

Again. If any thing depended on the Luminous Matter's being specifically *lighter* than Common Air, yet in the several degrees of Rarefaction approaching toward a *Vacuity*, there would be *Mediums* produc'd, approaching still nearer and nearer to the Specific Gravity of the luminous Matter. And consequently, as the Receiver is more and more exhausted, so the Fumes should be discharg'd in less plenty, and ascend with less velocity. Whereas on the contrary, they rise more copiously; and (the Light being more bright and vivid too) 'tis plain that they expand themselves, not with less, but greater force. And therefore I think the Gravity of the Air, as a *Medium*, has very little (if any thing at all) to do in this affair. But the Pressure or Gravity of the Air, as a *Pondus*, I believe, will account for it; and, as far as I can see, is the only Property that will do so.

I would remark here farther, particularly with regard to the third Experiment, that the *Phosphorus* Steams were apparently specifically heavier than the

the Medium produc'd in the Receiver by the last exsuction. For they did *descend* in that Medium. And from hence I may securely infer, that they did not ascend in that Medium by *Hydrostatical Laws*, but by the meer Impetus of their own Vibratory Expansive Motion, or the Force with which they were emitted from the Body which contain'd them, upon the removal of the Pressure which was before an Impediment to their discharge. For, that Impetus being spent, or overpower'd by their Gravity, they necessarily descended again by vertue of that *Law*, which obtains in all Portions of Matter of all sorts whatever.

An Account of some Experiments made about the Propagation of Sounds in Condens'd and in Rarefied Air.

EXPERIMENT I.

Concerning the Propagation of Sound in Condens'd Air.

A Bell being included in a *Brass Receiver*, was plac'd at one end of a Room about 50 yards in length: At the other end of which, some Persons stood to observe the Sound. Before any Air at all
 O was

was injected, the Bell (by shaking the Receiver) might be heard at that distance, tho' not without diligent attending to it. When one Atmosphere was injected (if I may take the liberty to use that expression,) the Bell being shaken as before, the Sound was observ'd to be very sensibly augmented. When two Atmospheres were injected, there was manifestly a much more considerable improvement of the Sound. But upon the intrusion of the 3d, 4th, and 5th Atmospheres, the Sound seem'd not to be augmented in proportion to what it was at the first and second. However, it was observ'd, that at this 5th and last injection, the Sound was very near as loud and sensible at the 50 yards distance, as it was when the Bell was struck in the open Air, without being inclosed in the Receiver at all.

Now the Reasons of the Sound's not proportionably encreasing in so much greater Condensations, I believe, may be these.

First, *The Deficiencies of the injected quantities of Air.* For the *Valve*, which should have hinder'd the return of the injected Air, might not perform its Office so exactly, or hold so tight as it should have done; and by that means some portions of Air might escape, and consequently the quantities injected not be so great as was suppos'd: from whence it would be no great wonder, that there should be a failure in the proportion of the Encrease and Propagation of the Sound.

Secondly,

Secondly, Tho' 25 Compressions of the Syphon are equal to the Natural Content of the Receiver; yet when the Air becomes pretty strongly condens'd (as 'tis by the intrusion of 4 or 5 Atmospheres,) the remaining Air at every stroke, which will lie between the bottom of the *Embolus* and the *Valve*, tho' it be but little, yet is of the same density, at that time, as the Air in the Receiver; which therefore, upon drawing up the *Embolus*, will extend it self to such a space of the *Cylinder*, as it can fill up by expanding it self into the state of common Air; and is so much as this comes to, of what should be injected at every stroke: 25 of which strokes, as I said before, are equal to the natural Content of the Receiver. And hence the Deficiencies of the real quantities, which should be injected by a certain number of strokes, may be very considerable; and to compute 'em, would be a business of as much difficulty.

EXPERIMENT II.

THE same Trial was made abroad in the open Fields, and with the same success as the former. Upon shaking the Bell before any Air was injected, the Sound was but just audible at 30 yards distance. When one Atmosphere was injected, it was heard as distinctly at 60 yards distance, as before at 30. Upon a second injection, the Bell might be heard at 90 yards distance. But after that, tho' near 100 strokes of the *Forcer* were repeated, yet it could hardly be heard 20 yards farther; which I attribute in great measure to the Reasons before-mention'd.

The time when this Experiment was made, was early, about five in the Morning, in the Month of *June*. The weather very *misty*, and little or no Wind stirring. And the silence requisite for the nice making such an Experiment, was by degrees interrupted by the Sounds of the five a-Clock Bells, and other noises from the City: all which in some measure contributed to the unsuccessfulness of the latter part of the Experiment. But this I hope some time or other to prosecute farther; not despairing in the mean time, of contriving such a *Gage*, as will shew the certain Quantities injected, without any danger or hazard in the Attempt.

EXPE

30

EXPE

EXPERIMENT III.

Concerning the Propagation of Sound in Rarefied Air.

HAVING included a *Bell* in a *Receiver*, which was shaken to make it strike, it was very observable that the interposition of the *Glass* betwixt the *Ear* and the *Bell*, was a great Impediment to the Propagation of the Sound, tho' it might be heard at a good distance from it. But the Air being gradually exhausted, and several stops made, to shake the *Bell* at the several different degrees of Rarefaction; I found that the Sound was remarkably diminish'd at each of those stops. At last, when the *Receiver* was very well exhausted, the Sound was so little, that the best Ears could but just distinguish it: it being like a small shrill Note, heard at a mighty distance. As the Air was gradually admitted into the *Receiver* again, so the Sound gradually encreas'd; this *augmentation* in the more *dense* Medium, answering by proportional degrees to the *Diminution* in the more *Rarefied* one. And when the *Receiver* was again replete with Air, the Sound seem'd something more clear and distinguishable, than it did when the *Bell* was first included, before any of the Air had been drawn out.

The Observation therefore to be deduc'd from these Experiments, is this, *viz.* That Sounds are augmented in Condens'd, and diminish'd in Rarefied Air: or, that That Undulating Motion in which Sound consists, is

is propagated with more facility and advantage in Condens'd, than in Common; and in Common, than in Rarefied Air.

And from hence we may infer,

Corol. 1. That the Distances at which the equally-strong Percussions of the same sonorous Body shall be equally audible to the same Ear, in Condens'd, Common, and Rarefied Air, (or, which is the same thing, in Airs of different degrees of Density,) must be taken in some proportion to the Densities of those several Mediums, thro' which the Sound is thus propagated. And that therefore, were that proportion establish'd by sufficient Experiments; from the Densities given, the Distances might be inferr'd; or from the Distances given, we might conclude the Densities requisite to make a Sound of a given Degree, to be equally audible at those given Distances. And therefore, were we to speak of the utmost Limits of Distance, at which any given Sound is audible at all; 'tis plain that these Limits must be determin'd by the same Law of Proportion concerning the Densities of the Mediums. Because the utmost Limits, at which any given Sound is audible at all, in any given Mediums; are likewise the Distances, at which that same Sound is equally audible in those Mediums. For when a Sound is but just audible in any Mediums, 'tis then equally audible in those Mediums.

Corol. 2. The Distances at which the different or unequally-strong Percussions of the same sonorus Body shall be equally audible to the same Ear, in Mediums of different Densities, must be taken, in some proportion, compounded of the strengths of the Percussions, and the Densities of the Mediums. And universally, to have Sounds

Sounds (*ceteris paribus*) audible or distinguishable in any given ratio; will require some composition of the Proportions of *Distances*, *Densities*, and *Forces of Percussion*.

Corol. 3. Speaking strictly, Sounds are not at all times equally audible to us here upon the Surface of the Earth. I mean, the like Percussions of the same sounding Bodies, are not at all times to be heard with the same facility, at the same distances. The Reason of which is sufficiently manifest, since the state of the Atmosphere here about us, undergoes such frequent Vicissitudes, (and those sometimes very considerable ones too,) as to Rarefaction and Condensation.

Corol. 4. Sounds diminish or become less audible, as we ascend upwards from the Surface of the Earth: And therefore in the upper Regions of the Atmosphere, and especially in those where the Planets revolve, sonorous Bodies must be at a distance almost infinitely near, (that is, in contact with the Organ it self;) or the Force with which they are struck, almost infinitely great; for Sounds to be equally audible, with what they are here upon the Surface of the Earth. The Reason of which is plain, from the prodigious Rarefaction of the Medium in those Regions. A Globe of such Air as we have here at the Surface; if plac'd at the height of a Semi-diameter of the Earth, would expand it self at such a rate, as to fill all the Planetary Orbs as far as that of Saturn; nay, and a much greater space than that. And That Medium, in which the Planets perform their Revolutions, is so fine and rare, as that its Resistance is wholly imperceptible, though they have gone their Rounds in it for so many Ages. To what a degree of Rarefaction then does the Medium

Medium arise in those sublime Regions? And what perception should we have there of such Sounds, as are here propagated to our Organs with a great deal of ease and force? For, the strokes of the sounding Body being equally forcible, the distance of the Organ must be less, in a rarer Medium, in some proportion to that rarity; that the Sound may be equally audible, as in a Denser: And the Distances being the same, the Strength of the Percussions must be proportion'd to the Rarity of the Medium, in order to produce the same Effect: And consequently, when the Rarefaction of the Medium is (as it is in those Regions,) so vastly transcendent to what 'tis here on the Surface of the Earth; an Organ so made as ours is, must either approach almost infinitely nearer; or the Sonorous Body must be struck with a force almost infinitely greater; that a Sound may impress the Organ there, equally with what it does here. The crackling of Thorns in a Fire, would shake our Ear with a vastly-more considerable force here, than the largest Cannon, or the most dreadful Claps of Thunder, would do there; were either of them discharg'd at a much less distance from the Hearer than what we are now speaking of.

The *Musick of the Spheres* therefore is an Entertainment, which we ought to despair of ever hearing: and That *Consort*, be it as Celestial as it will, yet wants a fit Medium (if *that were all that is wanting*,) to convey it to us. The old Philosophers were much in the right, in saying these fine Sounds were never to be heard; and as much in the wrong, in laying down that for the Cause, that the Noise was too strong and overwhelming to the Organ, for us to have any perception of it.

Corol. 5. *The Diminution of Sounds in Ascents or Elevations above the Surface of the Earth, will be in some proportion to the Descents of the Mercury in the Barometer at those Elevations.*

For were the Diminution of Sounds exactly in a *simple or direct* proportion to the Rarefaction or Expansion of the Medium, at any heights in the Atmosphere; that Diminution would be exactly in a *simple reciprocal* proportion to the heights of the Mercury in the Barometer at those Elevations: because the Expansions of the Air are found to be reciprocally as the heights of the Mercury. And therefore if the Diminution of Sounds be in some *complicate direct* proportion of the Expansions of the Medium, it will be also in some *complicate reciprocal* proportion of the heights of the Mercury in the Barometer. And consequently the Barometer might be made use of, to discover and determine the Diminution of Sounds in any Region of the Atmosphere; provided it were well determin'd by Experiments beforehand, in what proportion Sounds diminish according to the Rarefaction.

An Account of an Experiment, concerning the Refilition or Rebouncing of Bodies, in Various Mediums.

I Provided a tall *Glass Receiver*, in the upper part of which I had a Contrivance for the lodgment of four *Marbles*, (such as are generally sold at the Shops,) and from whence I could let them drop down on a *Plane* at pleasure. The distance from the *Plane* to the place where the Marbles were lodg'd, (and consequently the space of their descent,) was about $13\frac{1}{2}$ inches. And as to the bulk of them, two of these Marbles weigh'd 59 grains; and the other two, 63 grains. The *Plane* on which they were to fall, was a round flat piece of solid *Glass*, about 1 inch thick, and $3\frac{1}{2}$ inches over; the upper *Surface* of which was very well ground and polish'd. It was fix'd in a *Tin Frame*, contriv'd on purpose to keep its lower *Surface* from being contiguous to the *Plate* or *Leather* on which the Receiver was plac'd: the reason of which Contrivance was, to prevent an Inconvenience which would otherwise arise; as shall be shewn by and by.

All things being thus provided, the Marbles were dropt in *Common Air*; that is, in the Air included in the Receiver. After this, the Air was exhausted, and they were dropt in *Vacuo*. And then an Atmosphere of Air was injected, besides the natural Content of the Receiver, and they were let fall in that Con-

Condens'd Air. I say, *one Atmosphere*; for I did not dare to venture more, lest the breaking of the Receiver (which would be a hazardous thing) should have been the Consequence of it.

Now I found, that the Resilition of the Marbles dropt in *Vacuo*, was something more than that of those in *Common Air*: And those let fall in *Common Air*, had some advantage in their Rebound, above those let fall in *Air Condens'd*. The Rebound in *Vacuo* was about $10\frac{1}{2}$ inches, (which was more than $\frac{1}{4}$ of their Descent.) In *Condens'd Air*, it was about 10 inches. Accordingly, in *Common Air*, we must count the Resilition to be a *Mean* between the other two: For 'tis extreamly difficult to determine to a Nicety in a Motion so sudden, and of so short a duration. But this however is certain, that there was a sensible difference between the Rebound of those dropt in *Vacuo*, and those in *Condens'd Air*. As for the difference of the Weight of these Bodies, I could not find that That made any discernible alteration in their Reflexion.

I would give one Caution here, which may serve to prevent those, whose Curiosity may lead them to make these Experiments, from falling into an Errour, which I my self very narrowly escap'd.

The Glass (as I said before) was fix'd in a Tin Frame, on purpose to keep the lower surface of it from being contiguous to the Plate or Leather, on which the Receiver was plac'd. For when I first try'd these Experiments, I us'd a Stone Plane, laid carelessly upon the Leather which cover'd the Plate on which the Receiver stood: And accordingly, the *Air being exhausted*, the Marbles would not rebound so high by an inch, as when the Experiment came to be made

on the same Plane in *Common Air*. The reason of which was plainly this: That the Air being exhausted, the Leather consequently swell'd, and by that swelling rais'd the Plane which lay on it; and so, causing it to lie more soft and hollow than when 'twas only in *Common Air*, by this means the Resilition became less in *Vacuo*, than in *Common Air*: and the Event of the Experiment prov'd quite contrary, both to what it ought to have been, to what was expected, and to what after came to pass. For having fix'd the *Apparatus*, as before mention'd, all things succeeded then, both according to *expectation*, and to *Philosophical Theory*.

Corol. 1. In any exact Computations therefore of the Resilitions of Bodies, Account must be taken of the State of the Ambient Medium: For the Rebounds of the same Body will not be the same, in all the various conditions of that, as to Rarity and Density.

Corol. 2. Here's a manifest Proof of the Air's Resistance.

I know there are many other Proofs besides; but, I say, this Experiment also furnishes one. For the Difference in the Rebounds is no otherwise possible to be accounted for, since the Experiment may be rely'd on as made to a sufficient degree of Nicety.

Some

Some farther Experiments concerning the Electricity and Light, produc'd from various Bodies by Attrition.

EXPERIMENT I.

Being a farther Improvement of one made before, to the same purpose.

IT has been shown in one of the foregoing Experiments, how Bodies included in a Glass, might be affected with a very sensible Motion, by the bare approach of one's Finger near the outside. I have here something to add to the Account of that *Surprizing Phenomenon*, which will render it more wonderful still: And the *Appearance* in this Trial was so much the more conspicuous, by how much the *Apparatus* made use of was better contriv'd and adapted than in the former.

I observ'd then,

That when the Motion and Attrition of the Glass Plate VII. had been continued about two or three minutes, Fig. 6. and then ceas'd; the Threads seem'd to hang in great disorder, and without any degree of erection at all, for some small time. They continued in this posture (as near as I could count) for about three

three or four seconds, and then they were extended every way towards the Circumference of the Glass; and that with such a strength, that the Motion of the Glass alone would not very much affect 'em. But the strangest thing of all, was to see, that a Motion might be impress'd upon them by the approach of one's Finger, Hand, or any other Body, at more than three inches distance from the outward surface of the Glass, tho' the Threads themselves did not touch the inward one.

I observ'd further,

That every time the Motion of the Wheel and the Attrition of the Glass were repeated, the Threads might be mov'd, by the approach of ones Finger on the outside, at a still greater distance.

Nay, I have found since, that by blowing with ones Mouth only towards the Glass, at three or four foot distance, the Threads would have a very considerable motion given 'em.

And when I have suddenly spread my Hands upon the upper and lower parts of the Globe, there has been a violent Agitation of the Threads within, which has also lasted for some time.

From these Observations we may gather,

First, That the Cause of the Erection of the Threads (whatever it be,) tho' certainly excited by the Motion and Attrition of the Glass, yet does not necessarily work its Effect immediately, upon that Motion and Attrition.

For we see the Threads were quite loose and motionless for three or four seconds of time; and then they were extended, like so many Radii, towards the Circumference of the Glass.

'Tis

[III]

'Tis worth enquiry here, Whether the space of time between the *Cessation of the Motion*, and the *beginning of the Erection of the Threads*, will be the same in all Seasons, and in all Conditions of the *Ambient Air*. As also, Whether the longer or shorter continuance of the *Motion and Attrition of the Glass*, before they cease; does contribute any thing to the lengthning or shortning this Time of the *unactive state of the Threads*, before they begin to be erected.

Secondly, 'Tis manifest, there's a *Communication between the Medium without, and That within the Glass*.

This follows from the *Motions and Tremblings of the Threads*, upon the approach of other Bodies posited on the outside.

Thirdly, Not only a *Communication*, but a *Continuity*, of the Matter which occasions the Motion of the Threads. The Progress of it seems to be in a streight and direct track; in which the Matter is push'd by the *shortest Course*, from the *Approach'd Body* to the Threads that are shaken by it. And if the Threads are mov'd by influence of any Matter emitted from the Glass, it appears to be impossible to explain how they should be so, and at such distances, without a *Continuity*. So that the Case seems to be thus; That the *Effluvia* pass along, as it were in so many *Physical Lines*, or *Rays*; and all the Parts that compose them, adhere and joyn to one-another, in such manner, that when any of 'em are push'd, all in the same Line are affected by that Impulse given to others.

And for this purpose the following *Observations* deserve to be consider'd.

Observat.

Observat. 1. Having laid a piece of Leaf-Brass between two pieces of Wood, about an inch thick, and an inch asunder; I apply'd a well-rubb'd Tube as near as the Wood would permit; but the Brass receiv'd no manner of motion. But as soon as the Wood was remov'd, it was attracted vigorously, without any fresh Attrition of the Tube.

Observ. 2. When the Tube was well rubb'd; if a piece of Paper were immediately apply'd, so as to touch the upper part of it; the Leaf-Brass, scatter'd up and down upon the Table, would not be attracted at all, tho' the Tube were held very near. But upon removing the Paper, those Bodies were put into sensibly brisk motions.

Observ. 3. When a piece of Leaf-Brass is hunted about a Room; it keeps its distance, according as the Effluvia are more or less vigorously emitted: Nor will it by any means sink into the Sphere of the Effluvia, unless it meet a Body in its way; and then it will be attracted and repell'd several times, with a great swiftness.

Observ. 4. It may also be very properly urg'd upon this account too; that in the Experiment for producing Light by the Effluvia of the outward Glass, falling on the inward exhausted Glass in motion; after the Motions were ceas'd, a Light might be produc'd on the inward Glass, by approaching ones Hand near the surface of the outer one. Which seems convincingly to shew That Property of the Effluvia, I have been here speaking of.

Plate III
Fig. 1.

Observ.

Observ. 5. When the Tube was fill'd with some other matter than Air, the Attractive Power of the Effluvia was considerably abated.

Thus when I had stop't up one end of it with a Cork, and fill'd its Cavity with dry *Writing Sand*; tho' the same Attrition was made as before, yet the Leaf-Brass had no Motion given it, till the Tube was brought within an inch or thereabouts of it. But if the Sand were suddenly shot out of the Tube, then it would attract the same Bodies at double or treble the foremention'd distance, without any new Attrition at all.

This last Experiment compar'd with one before recited, makes way for a very considerable Observation: which is this.

I have formerly shown, That when the Air contain'd in the Tube was exhausted, the Attractive Power was quite lost, or very near so.

And here it appears, That when the Tube was fill'd with a *Heterogeneous Body*, the Attractive Power was exceedingly weakened.

Now in both Cases there was an *Exclusion of Air*; and in both Cases the same kind of Effect follow'd, viz. the *Loss of the Attractive Power*.

Only, where the Tube was exhausted, the Air being more perfectly excluded, the Attraction was also more remarkably lost, than when it was fill'd with Sand, by which the Air was excluded too, but not so perfectly as by the Exhaustion.

Now I take the Result of these two Experiments in conjunction, to be a signal Demonstration of the Influence and Interest of the Air, in these Phenomena. And if upon the filling the Tube with other sorts of matter (than what I made this Trial with,) the Effect still

Q

appears

appears to be the same; it must then pass for a Truth not to be disputed. Tho' on the other hand, if, when the Tube was fill'd with other Matter, the Effect should not answer in the same manner or degree, but the Attractive Power should be pretty strong and vigorous: yet this will be no conclusive Argument against the Interest of the Air in those particular Experiments, which I have here mention'd. For 'tis possible that other Matter may in one case yield that assistance towards the production of the Effect, which some Action or Operation of the Air may afford in another different Case.

And I think it may stand for a very useful Enquiry. *How far other sorts of Matter, with which the Tube may be fill'd, will any ways influence the Attractive Force of the Effluvia, so as to make any sensible alteration in it?*

EXPERIMENT II.

Concerning the Electricity of Sealing-Wax.

I Fitted a Wooden Cylinder (of about 4 inches diameter, and 3 in length) to an Axis, and plung'd it into melted Sealing-Wax, in which I kept it moving round till it had gotten a Coat of above $\frac{1}{2}$ an inch thick, on its Surface. The Wax was of the best sort I could procure; and the quantity melted was 1² lib. Having thus prepar'd the Cylinder,

I plac'd it on the *Machine*, and gave it the usual *Motion* and *Attrition*; which having been continued some small time, I held the *Hoop of Threads* (made use of in the *Experiment of the Glass Globe and Cylinder*) directly over it. The Effect was the same, as in those Experiments. For the *Threads* were directed by the *Attraction*, towards the center of that *Circle*, in the *Plane* of which the *Hoop* was placed. And while they remain'd thus directed, they would in like manner fly the approach of ones *Finger*. And *Leaf-Brass* would be strongly attracted and return'd, or carried about a *Room*, by the *Effluvia* of the *Wax*, as I have elsewhere shewn it would be by those of *Glass*. And the *Effluvia* of *Wax* likewise will be sensibly felt upon the back of ones *Hand*, if the *Wax* (after *Attrition*) be moved to and fro, near it; just as those of *Glass* may be. So that the *Electrical Qualities* of these two *Bodies* are the same, as to all the most *General Properties*. They differ only in *degrees*; the *Effluvia* of *Glass* producing more *Powerful Effects*, than those of *Wax*.

Plate VII
Fig. 3.

Concerning the Light producible from Sealing-Wax.

At *Night*, I gave the foremention'd *Cylinder* the same *Motion* I had given it in the *Day* (when I tried the *Electricity* of it,) and I applied some clean new *Flannel* to it; but there was very little or no *Light* at all produc'd by the *Friction* of these two *Bodies*.

Q 2

But

But when, instead of the Flannel, I applied *my naked Hand*, a considerable *Light* appear'd: the Properties and Circumstances of which (as far as I have observ'd them) I shall comprehend in the following Particulars.

This Light was visible only in that part, where the Attrition was made. The Light produc'd by the Attrition of the Glass Globe, was visible by its odd Flashes, all over the Surface of the Globe. It spread far beyond the part where the Attrition was made.

This Light depended most immediately upon the Motion; and would continue no longer, than that continued. Some Lights produc'd by the Attrition of Glass, have lasted for a while, even tho' the Motion has actually ceas'd.

None of this Luminous Matter would be communicated to ones Finger, when held near it. Whereas in the Lights produc'd from Glass, it was otherwise.

This Light produc'd by the Friction of the Hand, on the Wax, in the open Air; was hardly so considerable, as that produc'd by an Attrition made with Flannel, in Vacuo.

For That Light in *Vacuo*, was very discernable on each Arm of the Brass Spring, that embrac'd the Flannel. And could the Attrition have been made with ones Hand in that *very-rare Medium*, there's no doubt but the Light would have been still *much greater*. So that in this case there seems to be an Agreement between the Lights produc'd from *Wax* and *Glass*; viz. That both appear to more Advantage in *Vacuo*,

Vacuo, than in open Air; that is, in a very-weak and much-rarefied Medium, than in one of more Force and Density. And there's no Reason to believe, but all other Lights will agree in this Property too: Nor is it at all strange that it should be so, considering the fine and tender Nature of the Effluvia, on which these Lights, produc'd by Attrition, do depend.

Having thus shewn the Properties of this Light, produc'd by the Attrition of *Sealing-Wax*; I would subjoyn an Observation or two, which, I think, may deserve notice.

First, The Light produc'd by the Attrition of Sealing-Wax and Woollen, agrees exactly in one remarkable Property, with that which is produc'd by the Attrition of Amber and Woollen.

For they (both of them) disappear when the Attrition ceases.

Secondly, This Light agrees, in another Property, with some Light produc'd by the Attrition of Glass and Woollen.

For the Light of Sealing-Wax is confin'd to that part only where the Attrition is made. And so it is sometimes in the Friction of Glass on Woollen: For tho' the Light does many times spread all over, yet at other times it is limited only to that part which is rubb'd: as was observ'd in one of the foregoing Experiments.

Thirdly, This Light, and That produc'd from Glass, agree in another considerable Property, tho' in very different Circumstances of the Bodies themselves.

For,

For, if a *Glass Tube* be exhausted of its *Air*, the *Light* produc'd will not adhere to *Bodies* plac'd near it. Nor will any Parts of the *Light*, produc'd from *Sealing-Wax*, in the open *Air*, fasten upon *Bodies* which are fairly expos'd to it, tho' brought very near.

So that the same *Property* which, in one case, discovers it self in the open *Air*; in the other, necessarily requires a *Vacuity*, in order to the production of it.

So that the *Effluvia* of two different *Bodies*, (which otherwise do not agree in the same *Property*) may come to agree in the same *Property*, by the bare alteration of an external *Circumstance*, or by some change in the state of the adjacent *Bodies*. As here, by the meer *Substraction* of *Air* from the *Cavity* of the *Tube*, the *Luminous Effluvia* of *Glass* (which otherwise had the *Property* of adhering to *Bodies* plac'd near,) become now of that *Nature*, as not to adhere; wherein they agree exactly with those of *Sealing-Wax*, provok'd by *Attrition* in the open *Air*.

Query. Supposing *Lac* and *Vermilion*, to be the *Ingredients* in the *Composition* of *Sealing-Wax*; whether the *Attractive Quality* be owing chiefly to the *Former* or the *Latter*? Which will be answer'd, by trying the *Attractive Power* of equal *Bulks* or *Masses* of the same *Sealing-Wax*, made up with different *Proportions* of these two *Ingredients*. Ex. gr. Suppose I take any two quantities of *Lac* and *Vermilion*, and with them make a *Spherical* or *Cylindrical Body* of *Sealing-Wax*: And then for a second *Composition*, take either a greater or a lesser quantity of *Lac* than before; and mix so much *Vermilion* with it, as will make a *Spherical* or *Cylindrical Body*, of the same dimensions exactly as the former.

former: If the *Lac* be *more*, and I find the Attraction of the second mixture *stronger* than that of the first; 'tis plain that the Attraction is principally owing to the *Lac*: But if the *Lac* be less, and the Attraction be still stronger; 'tis plain that the Advantage of Attraction lies on the side of the *Vermilion*: Or *vice versa*. And so with respect to any other *Faëtitious* Bodies, we may discover what Ingredients (and in what proportion) do principally conduce to this Effect.

And the same Enquiry may be usefully made, with respect to the *Luminous*, as well as the *Attractive*, Quality.

And I should think it no inconsiderable step towards the advancement of our Knowledge of the True Nature of Bodies, to be satisfied upon what Principles or Ingredients in their composition, their *Light* and *Electricity* do mainly depend.

For this Point well settled, with respect to *Faëtitious* Bodies; we might be enabled to judge more truly of the Causes of the like Effects in *Natural* Compositions.

EXPERIMENT III.

*Concerning the Electricity, &c. of Sulphur
and Rosin.*

HAVING provided two *Wooden Cylinders*, of the same dimensions as that mention'd in the former Experiment; I coated their outsides, the one with *Sulphur*, and the other with *Colophony* or *Rosin* mix'd with *Brickduft*; which was added on purpose to bind the Rosin, and make it more hard.

Having given the *first* of these the usual Motion and Attrition, I brought it near the *Hoop fitted with Threads*; and found that *the Threads were attracted, and directed to its Centre*; but not near so strongly, as when the *Sealing-wax* was us'd.

And this, upon several Trials, was still much the same.

Then I try'd the latter, (*viz.* the Cylinder coated with *Rosin*,) and found that *the Threads were drawn to the Centre with more force and vigour*, even than when the Experiment was made with *Sealing-wax*: But this is to be added, that the Rosin having been melted, was not quite cold at the time when the Trial was made.

This is the main of the Experiment; to which I must subjoyn these following Observations.

First,

First, That when the Trial was repeated with these Bodies, the next day; *the Electricity of both was so inconsiderable, as scarce to deserve notice.*

Secondly, The Rosin, while warm, would attract Leaf-Brass, at the distance of an inch or two, without any Attrition at all.

Thirdly, In both these Trials, the Threads would fly the approach of ones Finger; but if Sealing-wax or Amber were held near them, *tho' they were neither of them rubb'd, yet the Threads would have a strong tendency towards them.* A Phenomenon I never observ'd any thing of before; and which gives a very surprizing instance of the *Attractive* and *Repulsive* Forces. That the Threads should be attracted by an Electrical Body, *while warm*, tho' the Attrition were ceas'd; (as for example, by the Rosin, while it yet retain'd a degree of Heat;) this is no great wonder: but that they should be attracted by such a Body, in a state wherein that Body was perfectly free from any degree of Heat, and without any preceding Attrition to excite and rouse the *Effluvia*; this I think has something very odd and peculiar in it. Nor do I think That centrifugal Motion of the Threads, upon the approach of a Finger, less surprizing. The Threads were altogether *in the same state*, when each of these Bodies, that *produc'd these very different Effects*, were plac'd near them. And yet they were *repell'd* from some of them, and attracted by others. But now, before that the *Amber* and *Sealing-Wax* on the one hand, or the Finger on the other, were brought near them; the Threads had been mov'd and acted upon,

upon, by those *Electrical Bodies* mention'd in the Experiment. From whence these two Queries.

First, Does that previous motion and excitation of the *Threads*, any way cause or contribute to this so very different an Effect, of their flying from one Body, and strongly tending to another?

Or, *Secondly*, Is the Reason of this Phenomenon to be entirely deduc'd from the Natures of the *Bodies themselves*, to which the *Threads* were expos'd? So that by Vertue of some Law or other, unknown to us, the *Threads* should tend towards This Body, and fly the approach of That.

Which of the two is the true Cause, I must leave at present to farther enquiry: And I think also it will not be very difficult.

Thus much for the *Electricity* of these Bodies. As to their *Luminous Quality*, I have but little to say.

Upon an Attrition of the *Rosin* in the Dark, I could find no Light at all.

And but very little from the *Sulphur*: And that not by a Friction made with my open Hand neither; but by holding the ends of my Nails very hard upon it, while it was in motion. And therefore, it either contains but a very small portion of luminous Matter in it; or else, That Matter is strongly retain'd within the Body of it; so that the ordinary degrees of Attrition are not sufficient to bring it forth.

I am apt to believe, that the *Latter* is more the true Reason, why so small a quantity of Light is producible from *Sulphur*; than the *Former*. For *Action* and *Re-action* are equal in all Bodies. Now as Light acts more upon *Sulphur* (and *Sulphureous Bodies*)
than

than it does upon any others; so reciprocally Sulphur acts more upon *Light*. And therefore, it being more strongly held in the Body of the Sulphur, by virtue of that *Law*; the *Emission* of it is much more difficult.

For such a Degree of Attrition, the Momentum whereof exceeds the Momentum of the Attraction of the contain'd Luminous Matter by the Body which contains it; is necessary to educe Light, or Luminous Matter, out of that Body.

And therefore Bodies which do with equal facilities emit their Light, should seem to have equal attractive forces on that Matter.

And Universally, The Attractions should be proportional to the Forces of Attrition, all other Circumstances being alike.

I tried whether Sulphur would emit any Light, by an Attrition in *Vacuo*: but with all my endeavours I could find none.

Now there was a very vigorous Light produc'd by an Attrition of *Sealing-Wax* in *Vacuo*; more considerable than that produc'd from the same Body in the open Air. Whereas the Effects of Sulphur were just the contrary: There was a small Light, produc'd with much labour, in the open Air; and none at all in *Vacuo*.

Query 1. Does the Absence of the Circumjacent Medium any ways contribute to the more strong Retention of the Luminous Matter in the Body of the Sulphur? If so; it has an Influence to produce the contrary Effect in the case of the *Sealing-Wax*, where a brisk Light appear'd when the Air was withdrawn.

Or, 2. Are the Effluvia of the Sulphur indeed emitted in *Vacuo*, as well as in the open Air; but not sensibly *Luminous* in that State of the Ambient Medium, that is, not *Visible*?

EXPERIMENT IV.

Concerning some very Uncommon Effects of the Effluvia of Sealing-Wax.

IN that Experiment, where the Threads are included in a Glass Globe, and, upon the Attrition of it, point every way from the Center to the Circumference, it was observ'd that, in that State, a Motion might be given to the Threads, by the approach of ones Hand near the outside. And this odd appearance, we know, is to be attributed to the Effluvia of the Glass, excited by the Attrition. For by some things formerly mention'd, it appears they are endowed with a Quality, which renders them capable of producing such Effects.

But I find, that the Effluvia of other Bodies, held without the Globe, will also perform the same Thing: Tho' the Threads are included there, and the Globe has no Motion nor Attrition at all given it; yet if another Electrical Body be plac'd near, they will move after a very strange and surprizing manner.

For

For they did so when I held rubb'd *Sealing-Wax* at the distance of 3 or 4 inches from the Globe. Also *Amber*, or a *Glass Tube*, would produce the same Effect.

Leaf-Brass, cover'd close with a flatbottom'd *Glass*, upon a Table; would have a brisk motion given it, by holding the rubb'd *Sealing-Wax* over it. And one single *Attrition* of the *Wax*, would be sufficient to keep those included little *Bodies* stirring for a considerable time. Nay, they have continued their Motion after the *Wax* has been taken away.

This shews the Penetration, Subtilty, and very great Activity of the *Effluvia* (at least of these) *Electrical Bodies*.

But 'tis to be noted here,

First, That this Experiment will not always succeed. Sometimes not at all; much less in that degree, I have here related. And the reason of this I take to be from a more Humid Temper of the Air, in which state some little moisture was probably condens'd upon the Surface of the *Glass*; and enough to be sure, might easily be, to obstruct the Passage of *Bodies* so fine and subtile as these *Effluvia*.

Secondly, This Inconvenience may be remedied, when it does happen. For if the *Glass* be plac'd a while in the *Sun-shine*, or a little warm'd by the *Fire*, or well rubb'd with a warm dry *Linnen Cloath*; then the *Leaf-Brass*, if the rubb'd *Wax* be held over it, will be put into as brisk motions as before.

Thirdly,

Thirdly, This warming or rubbing of the Glass, seems not only to clear it of the moisture, that might be condens'd on its Surface; but also, by actuating the parts of the Glass themselves, and perhaps raising some little quantity of Effluvia from it, encreases the force of those of the Sealing-Wax, and renders their action on the included little Bodies more considerable.

This I conclude from hence, viz. That when I had us'd any of the foremention'd methods, to clear the Glass from any thing of Soil or Moisture it might have contracted; I found I could give a Motion to the Leaf-Brass, only by rubbing my Finger on the outside of the Glass, without any assistance from the Wax. But yet when the rubb'd Wax was held over it, the motion of the included Bodies would be much more brisk.

However, when the Air is warm and dry, I never found any occasion to do any thing to help forward the Action of the Effluvia; their passage being then sufficiently clear; and the Bodies within, shewing by their various agitations how much they lie expos'd to their Power.

*An Account of the Success of an Attempt to keep
several Atmospheres of Air condens'd in the
space of one, for a considerable Time.*

I Took a very *strong thick Flint-Bottle*, which I had procur'd to be made on purpose for this Experiment; into which I injected with my *Syringe* between 4 and 5 Atmospheres of Air; as an included *Gage*, of about $4\frac{1}{2}$ inches in length, plainly enough shew'd. For, the Mercury rising up so far, as to fill about $\frac{2}{3}$ of the whole *Gage*, consequently compress'd the Air in the upper part of it, into nearly $\frac{1}{3}$ part of the space it possess'd before. This Air continued in that state of violent condensation from *March* the 30th, till about the 7th of *August* following. At which time happening to look on it (as I usually did once in 4 or 5 days) I found that the *imprison'd Element* had made its escape. Nor was I at any loss for the Cause hereof, when I consider'd the *intemperate heat of the weather* for some time before. For one day especially, I observ'd that the *Spirit in the Thermometer* had ascended 120 degrees above the *Freezing Point*. This hot state of the Ambient Medium, was sufficient to produce the foremention'd Effect; and to render the *Cement*, by which the *Brass-Cap* of the *Bottle* was fasten'd, (*even tho' it was preserv'd, for the greater security, under Water,*) to make it, I say, so soft and yielding, as not to be able to resist the Efforts of the mighty Spring of the inclos'd Air. By this means, all those parts of it, whose Springs preserv'd their *Tone*, readily exerted.

ted themselves, and got away out of the Bottle, leaving others behind them which were not able to unbend and consequently to gain themselves liberty. For I found that the *Mercury* in the *Gage*, continued still about $\frac{1}{4}$ of an inch above the surface of that wherein the open end of the *Gage* was immers'd; by which it appear'd, that the Air in the upper part of the *Gage*, still remain'd compress'd into a space, which was about $\frac{1}{6}$ part less than what the same bulk took up before the injection. But what deserves most particular consideration is, that *the Mercury still kept the same height, after its Surface in the Bottle was expos'd to the open Air.* So that those remaining parts of the Air inclos'd within the *Gage*, though they had all the Scope and Freedom possible, to expand themselves, yet did not do it; and therefore were some way or other render'd incapable of so doing. Had they been as able as the others, which went off before; they had likewise gone off too. So that their long detention in that violent state, must needs have made them unable to unwind themselves, so far as was necessary to their own discharge. And had not the foremention'd Accident happened, but they had continued in the condition they were in at the first injection; *there's no doubt but the disorder they suffer'd would have been still greater, and their incapacity of Restitution considerably more.*

Thus much for the Experiment it self.

And tho' the success of it was not so perfect as might be wish'd for, yet 'tis sufficient to inform us,

That *Air by long and violent Compressions, may (to all appearance) be depriv'd of much of its Elastick Power.*

That the Self-restoring Quality of those *fine Springs* (which in many instances produce such wonderful effects,)

Effects,) is so far impair'd by their being *held bent* for a long time together, that afterwards they do not sensibly exert themselves, tho' plac'd in the most *favourable and likely Circumstances* for so doing.

Thus we see, that that portion of Air which was left in the Gage, was not able to depress the Mercury, tho' the Surface of the Mercury in the Bottle was now expos'd to the open Air. The *injur'd Springs* could not recover themselves to their former *tone and temper*, but continued in a sluggish rest upon the Mercury, even after way was made for their *free and easie Expansion*.

Now if this was the Effect of the Condensation of *less than 5 Atmospheres* of Air; the greatest part of which, made its escape; and the longest time that any part of it was held in this state of violence, was *little more than 4 Months*; what would it be if 9 or 10 *Atmospheres* were crouded into the room of one, and continu'd in that condition for *as many years*, as the other did *Months*? Would not this Air lose its Elasticity much more than the former? *Would its tender Springs be able to unwind themselves much, after so long and powerful a restraint?*

Would not the Mercury keep its Height and Station in the Gage, notwithstanding that the Vessel containing Air so compress'd as this, should afterwards come to be expos'd to the open Element?

What kind of Liquid would so many Atmospheres of condens'd unelastick Air, compose?

Would not Terrestrial Animals be suffocated in it, as they would be in Water or other Fluids?

S

Would

Would not Fire in like manner be quickly extinguish'd, and perhaps with some noise and hissing, if put into such a Fluid as this?

Might not light Bodies (such as thin Glafs bubbles) float upon such a Medium; supposing them not above 5 or 6 times specifically heavier than common Air?

Is it impossible that Air by this means should become a Visible, Palpable Fluid; and be Subject to some of the same managements that other Fluids are?

What would be the consequence of the action of an intense Heat upon Air thus compress'd and depriv'd of all its Spring? Would it rarefie, and at last recover its Elasticity again, by the Changes possible to be wrought by the long continued action of Fire? Or, would its parts be only violently hurried about, as those of other Liquids are by the like cause, which afterwards settle and compose themselves again?

An

*An Experiment concerning the Production of Light
in an exhausted Glass (lin'd within-side with
Sealing-Wax,) upon an Attrition made without.*

HAVING procur'd a Glass Globe, of about 6 inches diameter; I put into it a convenient quantity of broken *Sealing-Wax*, and held it over a moderate Fire till the Wax was melted. Then turning the Globe about, that the Wax might slip from one place to another; it had quickly got a pretty thick Lineing, on more than half its inside: But 'tis to be observ'd, that it was not in all places equally thick, it being impossible to manage the melted Wax in such a manner as to make it so.

Having done thus, I plac'd the Globe in a fit posture, and left it till it was perfectly cold; and then having fix'd the *Brass-work* to it, I exhausted it of its Air. It was (immediately upon this) applied to the Machine, represented in Plate VII; where the manner of giving motion to it, is so obvious, as needs no description; and then making an *Attrition* with my Hand, I observ'd the following surprizing Phenomenon, which the Evening (the proper time for such Observations) permitted me to do with great advantage.

My Hand was no sooner applied to that part of the Globe which was lined with the *Sealing-Wax*, but I saw the shape and figure of all the parts of my Hand (which touch'd the convex Surface of the Glass) distinctly and perfectly upon the concave Surfaces of the Wax within. When the Glass alone,

without any such Lining on the inside, is made use of; 'tis obvious to any one (who has seen or does but consider the Experiment) how plainly a Hand must be seen, which is plac'd on the convex Surface of a Globe all over enlightned with a strong - flashing Light. And perhaps it may seem strange, if I should say, that *the Appearance was now as plain and visible as then, notwithstanding the interposition of the thick body of Wax.* 'Twas as if there had been only pure Glass, and no Wax in the way; or as if the Glass had been away, and the Wax were transparent. This Lining, where it was spread the *thinnest*, would but just allow the sight of a Candle through it *in the dark.* But in some places, it was at least $\frac{1}{8}$ of an inch thick. And yet even in those parts, *the Light and Figure were as distinguishable* as any where else. Nay, tho' some parts of the Sealing-Wax did not adhere so close to the Glass as others, yet *the Light appear'd on these, just as on the rest.* This Light produc'd, was *not discernible at all thro' the body of the Wax,* but was to be seen by looking thro' the other parts, where the Glass was free and transparent. The Colour, and other Properties of it, resembled those of *the Lights produc'd from pure Glass;* except in this one Instance, *That upon the admission of a small quantity of Air into the Globe, the Light wholly disappear'd in that part cover'd with the Sealing-Wax, and not in the other.*

When all the Air was let in, the Hoop of Threads being held over the Glass, the Threads were attracted at greater distances, by that part which was coated with the Wax, than by the other. But even then, when all the Air was exhausted, the Wax would attract Bodies plac'd near the outside of the Glass: For in this case I found *the Threads had their Central directi-*
on,

on, tho' not so vigorously, as when all the Air was let in.

But this is further remarkable too, with respect to that state of the Air's absence; viz. that *the Threads would not be attracted, if held over that part of the Glass which had no lining of Wax on the inside*: whereas if they were brought within the Sphere of the *Effluvia of the Wax*, they would direct themselves towards it.

So that in the Course of this Experiment, there are these following things to be taken notice of.

First, *A distinct and lively representation of the Shape and Form of an Object, upon a solid opake Body, to which that Object was not immediately applied; and this by the Light produc'd upon the Attrition of another Body (solid, tho' not opake,) to which that Object was immediately applied.*

A Man would have been thought the Author of a very strange Paradox, that should have asserted this, and at the same time conceal'd the Experiment; which shews how and which way 'twas done.

Or, should it have been propos'd by way of Problem, thus: *To represent the Figure of an Object (plac'd behind an opake Body) upon the contrary side of that opake Body; and this without the help of Optick Glasses, or any foreign adventitious Lights*: perhaps the Solution might have been thought impossible; or, it may be, the very Terms of the Problem it self, absurd and contradictory.

For, the Body on which the Figure is to be seen, must be an opake one, (by the Hypothesis;) and the Object it self plac'd on the contrary side
to

to that which it is seen on : So that either the Light must *be transmitted thro' this Body*, and then *'tis not opake*, which is contrary to the Hypothesis ; or else the Light must *not be transmitted*, and then *no Figure could be seen* ; for all distributions of Light by *Optical Artifices*, are excluded by the first Supposition.

But we see, this is not only possible, but also plain Matter of Fact.

From whence, I think, it may be useful to observe, *That many odd Effects and Appearances, which we may argue very plausibly to our selves, against the Possibility of ; and seem to find downright Absurdities and Contradictions in ; may yet be brought about by the genuine Forces of Nature, acting in convenient circumstances, upon proper and suitable Bodies.*

And from hence ; That we do not, upon such occasions, proceed to conclude too peremptorily, what may or may not be done ; and think, that every *Difficulty or Apparent Impossibility* to us, is a *Real one* to Nature it self.

Secondly, *The uniform Clarity and Perspicuity of the Figure represented, thro' all the parts of the opake Body, (viz. the Wax,) on which it was seen ; being as visible in the thickest and grossest, as in the finest and thinnest parts of it ; and on those which lay out farther from the Glass, as on those which adher'd more closely to it.*

Thirdly, *A total Disappearance of the Light in all that part cover'd with the Wax, upon the admission of a small quantity of Air ; and its Continuance in the other parts of the Glass at the same time.*

Both

Both these last-recited Heads do also furnish something that looks pretty strange.

Here's a Figure transmitted thro' the most dense and compact parts of an opaque Body, with the same facility and advantage to the Eye, that it is thro' those which should seem the most easily pervious to the radiant Matter which is to form the Representation.

Again : Here's a *notable Distinction observable in the Lights produc'd*. They were such, that one and the same Cause *destroy'd* the one, and *left the other untouched*. The Air swept away all which arose from the *parts lin'd with the Wax*; whilst the other Regions of the Glass preserv'd their Light without any diminution.

Fourthly, *The more strong and vigorous Attraction, from that part of the Glass lin'd with the Wax, than from the other*: which was manifest by the extent of the Attractive Power, from thence to greater distances than what the other would reach to.

Fifthly, *The Attraction and Central Direction of the Threads to the Wax, even while the Globe was exhausted of its Air*.

This answers to a like Phenomenon of the Load-stone; whose *Effluvia* will work their Effect, even when the Stone it self is plac'd *in vacuo*. So here the Threads were push'd towards the Wax, when at the same time it was included in a Glass, whose Air was drawn out. But then here's this difference, that *the Threads were less vigorously drawn in this state, than when all the Air was let in*; whereas all *Magnetic*

tick Attractions are (at the least) equally strong in *vacuo*, as in the open *Air*.

Sixthly, *The Limitation of the Sphere of Attraction to that particular part of the Globe, which had the Wax on the inside, (during that state of the Air's absence.)*

These things thus observ'd; we may now reason a little upon 'em, in the *modest way of Enquiries*.

1. *May not one Body attract (and as it were imbibe) the Effluvia of another contiguous Body; especially when Motion and Warmth have made an easie passage for such Effluvia into the Interstices of that Body, whose attractive Power tends to fetch them thither?*

2. *Might not (therefore) the Sealing-wax, by virtue of that Law, incorporate with it self the Luminous Effluvia emitted from the contiguous Glass? Glass gives a free passage to the Effluvia of Sealing-wax: May not Sealing-wax (on the other side) as freely admit the Effluvia of Glass?*

3. *Supposing the Body of the Sealing-wax thus charg'd and replenish'd with the Luminous Effluvia of the Glass; Would it not in that state appear Luminous it self? Do not all Bodies that shine, do so by vertue of Lucid Matter lodg'd in 'em; and, in some degree, more or less forcibly darted from 'em? Why should not Wax, every where replete with shining Corpuscles, appear shining; as well as Wood charg'd with fiery parts, gives us the Sensation of a burning Coal; or Smoke thoroughly heated, that of a lively Flame?*

4. *What*

4. *What is it to be pellucid, but to transmit Light receiv'd? And does not the Wax thus transmit the Luminous Matter, attracted and imbib'd from the Glass?*

5. *Has not the Wax (therefore) in this state, a sort of Transparency? I say in this state: For the Property is limited to the present Circumstances of these Bodies concern'd in the Experiment?*

During the *Attrition*, there is an *Eruption* of Luminous Effluvia from the Body of the Glass.

Does not the Attraction take place, as soon as the Matter to be attracted is furnish'd by the contiguous Glass? Is not the Wax saturated with Light as soon as the Attraction commences? And when the Wax is saturated with Light, does it not then appear luminous? (that is, does it not communicate some parts of the Light receiv'd, to the circumjacent Medium?)

6. Since therefore the Sealing-Wax in this state, is not to be consider'd purely as an *opaque Body*, which opposes the Transmission of Light, (*as it really is in all other Circumstances*;) but as a Body every where pervious to the *Lucid Matter* emitted from the neighbouring Glass; May we not from hence conceive, how the Figure of an Object plac'd on the one side, may be represented on the contrary side thereof, (namely, that side which is turn'd towards the Eye of the Spectator?) Why should I not as well see my Hand plac'd on the Glass, whilst the Wax is thus open to the Luminous Effluvia; as see it when I place it behind any ordinary transparent Body whatsoever?

In a word; 'Tis plain matter of Fact, that *the Figure of the Hand is seen on the contrary side of the Wax.* And 'tis demonstrable from the very Circumstances of the Experiment, that *that Figure is not form'd there by any of the common ways of picturing Objects by Reflected or Refracted Light.*

The Figure therefore is *transmitted thro' the Body of the Sealing-Wax.* But no Species or Picture can be transmitted thro' an Opake Body while it continues to be Opake; that is, while it continues *impervious to the Rays of Light.* Therefore the Wax must, at that time, be in the *contrary state*; that is, *pervious to the Luminous Matter.* This Luminous Matter is originally emitted from the Glass in the *Act of Attrition*; but how it should pass from thence into the body of the Wax, without an *attractive force bringing it thither*, I cannot (at present) tell how to conceive.

7. *Qu.* Whether the Figure was not as distinctly form'd on the thickest, as on the thinnest parts of the Wax, upon account of the Quantity of Luminous Effluvia every where attracted in proportion to the quantity of attracting Matter? And whether, it was not upon account of the very small (comparative) difference between the distances of the farthest and nearest parts of the Wax, with respect to the Glass; that the Luminous Matter was pretty equally drawn to both, and so the appearance became (to Sense) equally distinct on both? Or that the vibratory motion of the Effluvia at their Eruption from the Glass, might bring them as well within the Attractive Sphere of the remotest, as the nearest parts of the Wax?

8. *Is*

8. *Is not the more strong and vigorous Attraction from that part of the Glass lin'd with the Wax, caus'd by the united attractive Forces of the Glass and Wax?*

An Account of several Experiments about the Ascent of Liquids, between the nearly-contiguous Surfaces of Bodies.

WHenever we give *Natural Causes* an Opportunity of exerting themselves in the *same* or *similar* Circumstances, we have reason to expect the *same* or *similar* Effects. If any Phenomenon be the Result of *such a Principle or Power* in Nature, upon *such or such an Application or Disposition* of External Matter; then, when the like Disposition is made again, there's little doubt of the appearance of the same Phenomenon. Some Effects indeed there are, plainly restrain'd and consign'd to some *particular Qualities* of Matter; as the Phenomena of *Light* and *Electricity*, (before discours'd of;) which won't succeed in all Bodies alike. Others depending upon a far more *general and comprehensive Cause*, require no more, in order to their appearance, than *fit Circumstances*, or a convenient Disposition of Bodies, with respect to one-another; and so, things being brought within the *Sphere* of that Cause on which such Effects depend, they are immediately produc'd of course, by some *universal establish'd Law* of Nature.

Of this *later* sort (if I do not greatly mistake,) are *those Phenomena which we have now under Consideration.*

The Experiments made upon the Ascent of Liquors in small Tubes, gave me an occasion to think, what Varieties might occur upon the making the Experiment after a manner different from what before had been us'd : And what Success I have had in these Trials, I have here given a large and particular Account of, under the following Heads. In all which the *Philosophical Reader* will discover an *exact Uniformity of Appearances and Effects*, consequent upon the *similar Circumstances and Conditions of External Bodies.*

Here were no *small Tubes* made use of in any of these Experiments : But when Bodies were plac'd together in such a manner, that something equivalent to *small Tubes* would necessarily result from their *very Position*, with respect to one-another ; then the same thing always came to pass, that would have done had *common small Tubes* themselves been made use of.

EXPERIMENT I.

Of the Ascent of Liquor between two Glass Planes, in the open Air.

I Procur'd a couple of *Glass Planes*, which were part of a broken Looking-glass, being about 7 inches long, and $1\frac{1}{2}$ inch in breadth. Now tho' these, when clapt together, were very close, as seeming to touch in many parts, yet when they came to be immers'd in a Liquid, it would ascend between 'em : as was manifest upon their separation, when they were found

found actually wet on all their parts. But this Liquor being so thin and colourless, the Ascent of it between the Planes was not so easily discernible. Wherefore, to make it more obvious, I put a small piece of Paper on each corner, by which means they were separated by an Interval equal to the thickness of the Paper, when they came to be applied to one another. This done, I plung'd one end of them under the surface of a strongly-tinged Liquor; upon which it began immediately to ascend, but not with that swiftness as in small Tubes: However, the Motion of it was very odd, being sometimes higher in one part than in another, and shooting out very pleasantly into diverse Branches; which it continued to do, till it had reach'd its greatest height.

But the Height of its Ascent varied according to the distance of the Planes. For if instead of one piece of Paper on each corner, two were laid there, the Liquor would not mount so high in the later case, as in the former when the Planes were separated only by a single Paper. And then, if the Planes were any ways declin'd, the Liquor would still spread it self farther and farther, in proportion to the degree of Declination.

And, upon several Trials, this all succeeded much after the same manner.

EXPERIMENT II.

The same in Vacuo.

BEing willing to try the Ascent of the Liquid between the Glass Planes, in an *exhausted Receiver* as well as in the *open Air*; I fix'd the Planes so to a *Brass Wire*, (which pass'd thro' the Cover of a *Receiver*,) that I could make 'em descend at pleasure. In this manner I convey'd them into the *Receiver*, together with a *Dish of tinged Liquor*; which having plac'd on the *Pump*, the *Gage* in a little time shew'd the Air to be pretty nicely drawn out. Then I plunged the Plates (separated by pieces of *thin Paper*, as before,) into the *Liquor*; which arose between them, as in the *open Air*. Nor was there any other difference than only this; That there appear'd more *Intervals or Spaces between the Branchings of the ascending Liquid*, than when 'twas try'd in the *open Air*.

However, when the *Air* came to be let in again, those *Spaces* were fill'd up with *Liquid*; which was now an *entire Body*, without interruption.

EXPERI-

EXPERIMENT III.

The Ascent of Liquids, between Marble and Brass Planes, in the open Air.

I Procur'd a pair of *Marble Planes*, ground as true as the Workman could possibly make them: These I joyn'd together *dry*, and *without any thing betwixt 'em*; which having done, I *immers'd* the lower Edge of 'em about $\frac{1}{4}$ of an inch *below the surface of the Water*, and held 'em there for some minutes: Then taking 'em out, I found I could not easily part 'em, without *sliding* 'em one off from the other. But having separated 'em that way, I presently found how far the Water had *infinuated* it self *betwixt 'em*.

This Ascent of the Liquor I found, upon various Trials, to be different; but always observ'd, that when I had newly rubb'd the Planes over with *Wood-asbes*, the Water would ascend the highest.

After this, I made use of a pair of round Brass Planes; which having order'd as before, the Success was very agreeable to what it was in the former Case.

And there's little reason to doubt, but the same thing would happen, if any other sort of Bodies were us'd, whose Surfaces are very plain and smooth, and posited so, as to be nearly contiguous to each other.

EXPERIMENT IV.

*The Ascent of Liquors between two round Glass
Planes, in the open Air.*

I Laid these round Planes one on the other, without any thing at all to separate 'em; and having plunged the round Edge just under the surface of the tinged Liquor, I observ'd, *That it immediately spread it self thro' the whole surface of 'em, and reach'd the extream parts.*

In the other Cases (before mention'd,) a *streight flat* Edge was applied to the Liquid; but *here* only a *circular* one; so that fewer parts of the Glass were dipt in this Trial, than in the former where *square* and *oblong* Planes were us'd. Notwithstanding which difference, the Water mounted upwards, and that in as little time too, as in the former Experiments.

EXPERIMENT V.

*The Ascent of Water thro' a Tube fill'd with Ashes,
in the open Air.*

I Took a Glass Tube, whose length was 32 inches, and the diameter of its Cavity near $\frac{1}{4}$ of an inch. To one end of this Tube I ty'd a *piece of Linnen Cloth*, and then fill'd it with *Ashes*, which had been *sifted thro' a pretty fine Searse*. As I put in the Ashes
by

by *small quantities* at a time, I ramm'd them down strongly with a *Rammer*, whose *Basis* was very little less than the *Bore* of the Tube ; by which means I *crouded* them together as close as was possible. When the Tube was full, I ty'd over that end of it (by the *Neck*) a *thin limber Bladder*, (which I freed from all its included Air) in order to receive that Air, which I expected would be forc'd thro' the *Ashes* upon the *Ascent* of the Water. This done, I plung'd that end of the Tube (to which the Linnen was ty'd) under the Surface of the Water in a *Glass*; and found that the Water did presently begin to rise. The very first *Ascent* was pretty considerable ; For in the space of 16 Minutes it had got up near 1 inch and $\frac{1}{4}$. But as it *advanc'd* still higher, its progress was slower, and that in such Proportions as here follow.

At the end of 24 Hours, it had risen but to 16 inches; the *Bladder* at the top being then near half fill'd with the Air, which had quitted the *Ashes*, as the Water past thro' them. But here happen'd an Accident, which prevented any farther Observation of the *Swelling* or *Distention* of the *Bladder* by this expel'd Air. For the upper part of the Tube (to which the *Bladder* was ty'd) being crack'd round, soon after drop'd off. However, this hinder'd not the continuation of the Experiment with respect to the *Ascent* of the Water. For at 24 Hours from the last Observation, I found it had gain'd 6 inches more in height. And 'twas very easie to trace it in all its motions, by the change of colour the Water gave those parts of the *Ashes* it past thro', which render'd them very distinguishable from those which were yet dry.

When 24 Hours more were compleated, the Water had rise $4\frac{1}{2}$ inches, and something better. And at the *same distance of Time* again, it had ascend- ed 3 inches higher. The succeeding 24 Hours brought it 2 inches higher still; and now it was gotten within $\frac{1}{2}$ inch of the top of the Tube. In the space of 10 Hours more, it finish'd that *little remaining part*, and reach'd the Extremity of the Tube compleatly.

Such was the Progress of the Water, and at this rate did it make its way thro' the *compact Body of the Asbes* with which the Tube was fill'd.

Having finish'd this Observation, I then resolv'd to know, *what quantity of Water the Asbes had absorb'd*; in order to which I proceeded thus. I weigh'd a Glas of Water very nicely, and pour'd part of it into the Glas; wherein the Tube had all-along stood, till it reach'd the mark at which the Water stood, when the Tube was first immers'd in it. Then (weighing the remainder) I found the quantity of that pour'd forth (which was therefore equal to that absorb'd by the Asbes) to weigh 1792 grains; which is pretty nearly that of the bulk of 7 Cubical inches. Now the Capacity of the Tube (its Diameter being $\frac{1}{4}$ of an inch, and its height 32) was about 14 cubical inches: So that the quantity of Water equal to about $\frac{1}{2}$ the Content of the Tube, was drank up by the Asbes.

I shall now take notice of the Particulars occurring in this Experiment, which seem to me to be well worth observation: And these I shall offer here by themselves; intending to make some general Remarks upon this whole Class of Experiments, after I have related all the Experiments that belong thereto.

First,

First, Tho' the Ashes were ramm'd so very close together, yet the *Interstices* of them were capable of admitting a quantity of Water equal to half the Content of the Tube. For the Content of the Tube was but little more than 14 cubical inches, and the bulk of Water absorb'd was as good as 7; as was but now observ'd.

Secondly, The Progress of the Water thro' the Ashes, was very disproportional to the Times: Because 'twas found, that in the equal Intervals of 24 hours, it made its way according to the following Series; viz. 16, 6, 4 $\frac{1}{2}$, 3, 2, (and in the last 10 hours) $\frac{1}{2}$ an inch.

Thirdly, The Force with which the Water made its Ascent, was very considerable; being such as was sufficient to overcome the Resistance of the Air imprison'd in the Interstices of the Ashes, and to drive it away before it, towards the upper part of the Tube.

Now 'tis plain the Resistance of the contain'd Air was not a very small one, from hence, that it was superiour to that Force by which the Thorax is contracted, and the Air thrown out of the Lungs, in a strong Expiration. Because when I endeavour'd to force Air by my Breath, thro' the Tube not above half fill'd with Ashes, I could not presently satisfy myself that I did do it: Whereas we see the Water easily made it self a Passage, when the Tube was not only quite full, but also the Ashes were press'd together, as hard and close as possible.

But to put it out of all doubt, that the ascending Water did actually meet with and overcome such a Resistance as what I speak of, viz. That of Air lodg'd in the Interstices of the Body it pass'd thro'; let it be observ'd in the next place,

Fourthly, That it was visible by the gradual Intumescence of the Bladder at the top of the Tube, that the Air was really protruded out of the Ashes by the Water, as it ascended along.

I believe none will attribute this Smelling of the Bladder to any other Cause than the force of some included Air, which stretch'd it, and plainly endeavour'd to get away by so doing. And that it was forc'd out of the Ashes by the Water, is as obvious as any thing can well be; since there could be no other possible Cause that should expel it at that time. And besides, in that it gather'd more and more in the Bladder, as the Water advanc'd higher and higher, by that means it plainly pointed out the Cause which forc'd it thither.

Fifthly, The Water rose not only in the Ashes adjoining to the inward surface of the Tube, but also thro' the whole body of it, and that equally too, (as appear'd upon examination.)

Whatever therefore were the Cause of the Water's Ascent, that Cause acted uniformly, since the Water was in all parts and places equally influenc'd by it.

Sixthly, The Bulk of Air forc'd out of the Interstices of the Ashes, by the Water, we may conclude (and I think rightly) to be equal to that of the Water which supplied its place. And if so, then 'twas as much as half the Content of the Tube, or pretty nearly as much as the Bulk of Ashes therein contain'd, (as follows plainly enough from the first Observation.)

Seventh-

Seventhly, *The Ascent of the Water was by far swifter, when there was a much-greater quantity of imprison'd Air to oppose its passage, by reason of the longer Column of Ashes (in which that Air was contain'd) than when it had made more way, and (by getting higher in the Tube, having shortned the Column of Ashes) had a less quantity of Air to resist it in its motion upwards.*

Query 1. Does not this Phenomenon of the Ascent of the Water through the Interstices of the Ashes, amount to the very same Case with that of its rising in small Tubes, or between two Glass Planes? Do not the Particles of this Matter, by their little Hollows and Intervals, form a Congeries of minute slender Pipes, or Surfaces very nearly approach'd to each other; so that the Liquid rises in each Case by vertue of one and the same Cause?

Qu. 2. Why is the Ascent of the Water slower, the higher it rises in the Tube?

'Tis evident, that at first there is more intercepted Air to be remov'd out of the way, than afterwards, when the Water has shortned the Column of Ashes.

Is it therefore true, that the Water does at first actually meet with a more powerful Resistance, and notwithstanding rises with more Velocity, than when 'tis less resisted? Or, should we not rather conclude, that it does indeed meet with less Resistance at first, than afterwards; and therefore, that this intercepted Air is not in reality that Obstacle, which at first sight it appears to be?

Qu. 3.

Qu. 3. Whether the *encreasing Weight* of the Water, as it ascends, may not be esteem'd the Cause of the *diminution of its Velocity*? Because, from *Static Principles*, the same Power moving *different Weights*, should produce *different Rates of Velocity*.

EXPERIMENT VI.

The Ascent of Water thro' Ashes in Vacuo.

HAVING fill'd a Tube about 10 inches in length with Ashes, (as before) it was plac'd in a Receiver, and the Air exhausted. I suffer'd it to stand some time in *that state*, to give liberty to the Air contain'd in the Ashes to get away: Then plunging the lower end of the Tube under the Water, I found (according to my Expectation) that the Water rose much faster in that *very-much-rarefied Medium*, than in the open Air. Because, in about 4 hours time, it had mounted as high as it could go, having compleatly reach'd the top of the Tube.

So that comparing the Result of this Trial with the former, we find that Here was a Height of 10 inches surmounted in 4 hours; whereas There 32 inches took up 130 hours to finish it.

By which Account it appears, that the Heights are in the proportion of $3\frac{1}{2}$ to 1, but the Times as 32 to 1. So that the Water was more than 32 times as long in going (in *Common Air*) a Space triple to that, which was finish'd in *Vacuo*.

But

But this Estimate of the swiftness of the Water's Ascent would have been more exact, had it been observ'd, at what Time precisely the Water reach'd the same Height in both Tubes. Ex. gr. As here in *Vacuo*, the Tube made use of was 10 inches long; so if it had been observ'd, in the other Case, at what time the Water had reach'd 10 inches in that Tube also, (as it was observ'd at what time it rose 16 inches, viz. at the end of 24 hours) then the Proportions of these different Times in which the Water had ascended to the same Heights in both Tubes, would have given a nearer account of the Velocities. For if the Motions were equable, the Velocities would be just reciprocally, as those Times. But if they are not uniform, yet the rate of the Swiftnesses may be more nearly guess'd at, by taking the Liquid at the same Height in each Tube, than at different Heights.

EXPERIMENT VII.

The Ascent of Liquors in small Tubes, of unequal Thickness, but equal Bores or Cavities.

HAVING procur'd two Tubes, the Diameters of whose Cavities were as nearly equal as they could be made, but one at least ten times as thick as the other; I put them into the foremention'd tinged Liquor. The Result was, That there was no difference to be perceiv'd between the Heights, the Liquor had ascended to in each Tube.

EXPERI-

EXPERIMENT VIII.

The Ascent of various Liquors between two square Glass Planes.

I Try'd this in *Spirit of Wine, Oyl of Turpentine, and Common Oyl.*

All these rose between the Planes, as the *tinged Water* did. The Difference lay in this, that they all ascended in *an entire Body*, from one side of the Planes to the other, without *those Interruptions and Intervals*, which generally happen when the Water ascends. And this, even tho' the Planes were held together *without any thing to separate 'em*; and not only so, but also *forcibly press'd together*: in which case they must needs touch in many parts. And notwithstanding *that*, the Course of the Liquor seem'd to be perfectly uninterrupted.

There was a *remarkable Difference between the Times spent by the Spirit of Wine, and the Oyls, in their Ascents.*

The *Common Oyl* mov'd extreamly *sluggishly*, in comparison with the *Oyl of Turpentine* and *Spirit of Wine*; insomuch that the *former* was near an *Hour* in rising as high as the *two later* would do in less than *half a Minute.*

Having now given an Account of the Experiments themselves, and subjoyn'd what Remarks had a more *immediate relation* to any of them in particular; I shall now make some *general Observations* upon the whole, and then consider how the Phenomenon it self may be solv'd.

First,

First of all then, we find, that *this Phenomenon of the Ascent of Liquors*, (between the Surfaces of nearly-contiguous Bodies) like that in small Tubes, does no way depend upon any action or influence of the Air.

For in all these Trials, the Liquor rose with as much ease and freedom in an *exhausted Receiver*, as the *open Air*; but in one case particularly, it ascended with a vastly-greater velocity in so thin a Medium as that we call a *Vacuum*, than under all the Pressure and Vigorous Action of *common Air*. There was indeed some difference, with respect to the *branching and spreading* of the Liquor in its ascent; but this is a trivial consideration in comparison with what ought chiefly to be regarded in this matter, and that is the *Height and Force* of the Liquour; which *without* the Air, will be at least (to say no more than that) as considerable as with it.

Secondly; Some Liquids rise after a manner very different from what others do.

This is plain upon these two accounts.

1. Some Liquids, as they rise, *branch* themselves into various little Streams or Rivulets, and by that means leave (to all appearance) vacant spaces and intervals betwixt them; after which manner, 'twas observ'd before, that the *ting'd Water* rose between the Glass Planes. But others again mount up all in an entire body, from one side of the Planes to the other; as *common Oyl*, that of *Turpentine*, and Spirit of Wine.

2. Some ascend with a *prodigious swiftness*; in comparison of others.

X

Thus

Thus the *Two last mention'd Liquors* made at least *120 times as much haste* to get up between the *Planes*, as the former did (as is plain from what was before related of them.)

And perhaps *other Liquors* may be discover'd, which may as much exceed *these* in the velocity of their Ascent, as they did the *common Oyl*. And it may be, all *imaginable proportions of Velocity*, may be answer'd by those of *Liquors* (of some sort or other) ascending thus between the *contiguous surfaces of Bodies*, or in *small Tubes*. For the Cause of this Phenomenon (if it be what I take it to be) is capable of producing an *infinite diversity of Effects*, according to the difference of the *Matter* it has to work upon.

Thirdly; *Liquids ascend not only in perpendicular Directions*, but in all *imaginable Angles of Obliquity to the Horizon*.

For when the Experiment was made with the *round Planes*, the *tinged Liquor* immediately diffus'd it self, to the extremities or edges of them, every where thro' the *whole Circumference*. Now the *Liquid* could rise *perpendicularly* but in one Direction only, viz. that which we may conceive to pass thro' the *Center* of the two contiguous circular *Planes*. In all the other Directions it must ascend *obliquely*, diverging just as an *infinite number of Chords* in a *Circle*, drawn from the end of the same *Diameter*.

And supposing it reach'd all parts of the *Circumference*, at the same time (as it did without the least difference to Sense) we have then here as it were the Reverse of *Galileo's famous Proposition*, about the *Equitemporaneous Descents* of heavy *Bodies* in the *Chords of a Circle*: For in this case, the ascending *Liquid*

quid describes them all in *equal times*, as in that case, the *descending Solid* does. And if the *one ascends*, and the *other descends*, by virtue of one and the same Cause (as I cannot forbear thinking but they do); then 'tis no wonder that there should be such an Agreement betwixt them, and that the *same Cause* should produce a *similar Effect*, both in *Solids* and *Liquids*, when *similar Circumstances* are suppos'd on both sides. And it all amounts to no more, than *Attraction* upwards in one case, and downwards in the other; and this in the *same sort of Figure* too namely, a *Circle*.

Fourthly; This Phenomenon is not confin'd to any one particular sort of Matter.

The Liquids rose, not only between the *Glass*, but the *Marble* and the *Brass Planes* too. And there's no doubt, but had the Experiment been made with Planes of various other sorts of Matter, it might have succeeded in like manner. 'Tis possible, that *some Liquids* may not rise between the surfaces of *some Bodies*, which others will rise freely between: nay, I know not, whether *instead of rising*, they may not be *sunk and depress'd*. One and the same Cause, acting in *different Circumstances*, is capable of producing a *great variety of Effects*.

Fifthly; A greater quantity of Matter contributes nothing to the *rising of the Liquid*.

This is plain from the Experiment of the two Tubes of *equal Cavities*, but *unequal Thicknesses*. And by a *parity of Reason*, the Thickness or Thinness of the *Planes* should produce no alteration, with respect to the Liquor's ascent between them.

Sixthly ; The *Ascent of the Liquour* is favour'd and promoted by small Particles of Matter laid in its way.

Thus the Water *ascended highest*, when the Planes were rubb'd over with *Wood-Asbes*. Perhaps other Matter might be as great an Impediment ; or give more assistance to some Liquors in their Ascent, than to others. But these and many other things (which now I can but hint) may perhaps some time or other be propos'd as Subjects of further enquiry.

Having made these Observations (*General and Particular*) upon the several EXPERIMENTS propos'd, the next thing is the *Solution of the Phenomenon it self*. And here I make no scruple, to reduce all the Varieties mention'd to the *simple Case of small Tubes* ; because they all of them (as is plain by considering the Circumstances) amount to no more than *That*.

For example : The two *Glass Planes* in these Experiments, being plac'd *very near one-another*, compose a Tube of the Form of a *Parallelipiped*, whose thickness is exceedingly *small*. So that therefore, having found a *Solution for the Phenomena of small Tubes*, the same may easily be accommodated to all the rest.

To proceed then ; It appears evident to me, that the Principle we ought to have recourse to in this Case, is no other than that of *Attraction*.

A Principle which governs far and wide in Nature, and by which most of its Phenomena are explicable. I know very well there have been Attempts made, to solve this Appearance diverse other ways. Some have argued from the *impeded or diminish'd Action of the Air* ; others from the *Innixion or Resting of the*

the Parts of the Fluid, on the Pores and Asperities of the Glass; others again from the Congruity and Incongruity of the Parts of Matter one to another. This last Notion, without farther explication, is somewhat more unintelligible than the two former: And tho' perhaps they are all of them wrong, yet the First Two ways of solving the Difficulty have this advantage above the other, that they are perspicuously False; whereas this latter is more mysteriously so, leaving the Understanding in some doubt, whether it may be True, or no; because of the hard Words of Congruity and Incongruity, which being not explain'd, may possibly carry some better Meaning along with 'em than they seem to promise. If it should be thought that Attraction is a Word no less hard and unintelligible than the former are, I can only say this, That 'tis plain Fact that there is a Power in Nature, by which the Parts of Matter do tend to each other; and that not only in the larger Portions or Systems of Matter, but also the more minute and insensible Corpuscles. And that the Law which obtains in the Former Case (*viz.* amongst the greater Bodies of the Universe) is fully determin'd and settled, namely, that the Attraction or Centripetency decreases reciprocally, as the Squares of the Distances (of the Attracted from the Attracting Body) do encrease. But the Law by which the smaller Portions of Matter tend to each other, is not so compleatly settled, but left yet for further discovery; only 'tis known, that it must be very different from the other, and that the attractive Forces here do decrease in a greater proportion than that by which the Squares of the Distances do encrease: but the nature of that Proportion, or how complicate it is, or what Varieties there may be in it, is not yet accounted for; nor will not easily, because of the seeming invincible Difficulties
that

that attend the making Experiments and Observations requisite to settle so nice a Point. Only the Fact it self is past dispute, and the Discoveries made by that very great Man, Sir ISAAC NEWTON, (the Honour of our Nation and Royal Society) have set both these Laws of Attraction thus far, in a very clear Light to all that will use their Eyes to see them.

Now since we are certain there is such a Principle in Nature, and one so Extensive and Predominant too, as that of Attraction; I think it would be a satisfactory Proof enough of the Interest of that Principle in this Phenomenon, to shew that it may be handsomly accounted for by it, without being forc'd upon any of those obscure precarious Suppositions, which in other Solutions a Man can't well avoid.

However, before I do this, I shall argue the Point another way, and by shewing some remarkable Agreements of this Phenomenon, with others in which Attraction is most evidently concern'd, do something (I hope) to perswade a belief of the Interest of the same Cause here also.

What I propose to consider at present, is the Magnet or Loadstone, some of whose Effects coincide with those of small Tubes to a Wonder.

1. A Loadstone of any Form whatsoever, will attract Iron.

So we find that Bodies set together after any manner, or in any figure whatsoever, so they do but compose a small Tube (or what is equivalent thereto) will give occasion to the Liquor to ascend between their Surfaces.

2. The Magnet exerts its force as well in *Vacuo*, as the open Air.

And

And we find that *Liquids rise as freely in the one, as the other of these: in the most thin and rarefied Medium, as well as the most gross and dense.*

3. *Small Loadstones* (for the generality) have a *stronger* attractive Power (in proportion to their bulk) than the *large* ones have: And so *small Tubes* will make the Liquid ascend higher than great ones will. And as the inward *Cavities* and *Surfaces* are *lessen'd*, so the Liquid will rise *higher and higher.*

4. If a Loadstone be *divided into several parts*, or *small Loadstones*, *these little ones* (supposing the virtue of the Stone to be *equally spread thro' the body of it*) will *all together sustain a vastly greater weight of Iron*, than the *one great one* alone before would do; tho' taken *collectively*, they contain very nearly the *same quantity of Matter* with it.

So, were a Tube of a *very small Bore*, but a *great Thickness*, to be divided into several Tubes or *parallel Surfaces*, the Quantities of Water sustain'd in all of them together, would vastly transcend *that*, which was sustain'd when they were *all united together*, and compos'd but one single Tube.

So that in *small Tubes*, as well as *Loadstones*, the Encrease of Superficies is That on which the Encrease of the Force seems mainly to depend. Nor does this Assertion any way thwart what was said before, *viz.* That as the *Surfaces did decrease*, so the *Ascent of the Liquid* would be more and more considerable: For there I only compar'd the Effects of Tubes of *different Diameters* one with another; and shew, that the lesser Tube has the advantage of the greater: But here I compare the Effects of *innumerable little Tubes,*

Tubes, all made out of one single Tube, with the Effect of that single Tube it self; and shew, that the Increase of Superficies consequent upon such a division, gives the Aggregate, or Collection of Tubes, (by many degrees) the advantage of the single one.

Now, upon these accounts, I think I have some Grounds to believe, that the Phenomena of the Load-stone, and of small Tubes, depend upon one and the same Principle in general: For here's a surprizing Correspondence of Effects, why then may they not agree in their Cause?

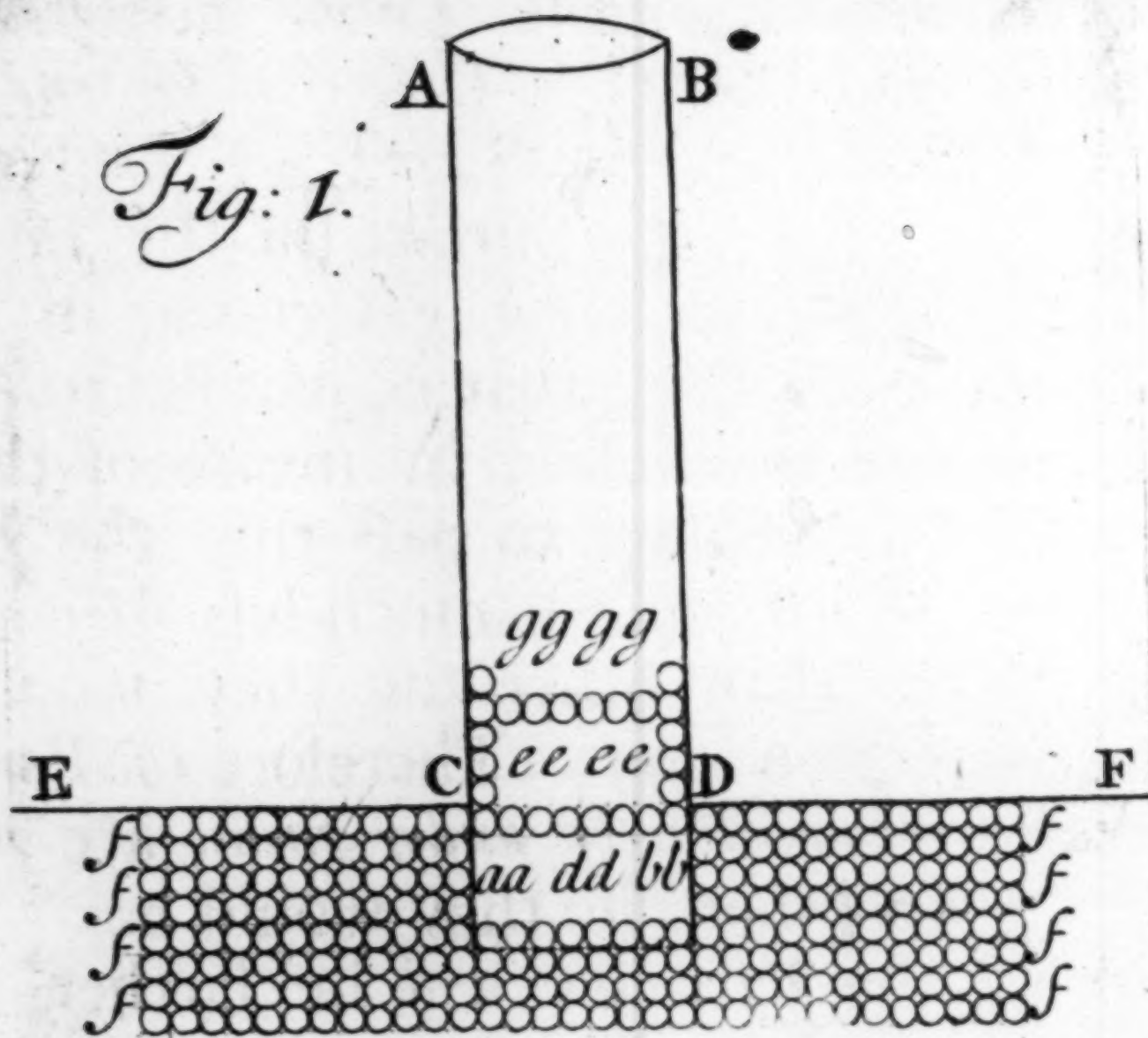
But to proceed. That *Attraction* I speak of (as the Cause of the Ascent of Liquids in small Tubes) I make to proceed (mainly, if not solely) from the Innermost or Concave Surface of such a Tube; and not from the Solidity or Quantity of Matter which it contains. And a Proof that this is so in Fact, may be deduc'd from the Experiment of the two Tubes, before mention'd. But more than this, that it likewise ought to be so, appears from hence, that the Attractive Power of small Particles of Matter acts only on such Corpuscles as are in contact with them, or remov'd but at infinitely little distances from 'em.

Upon which account I think I may say, that the remoter Surfaces of the Tube, between the innermost or concave one, and the outermost or convex one, do contribute nothing to the Effect; that is, the Liquid is not influenc'd by any Attraction of Theirs.

These things premis'd, let us in the next place consider, how this Phenomenon may be accounted for by Attraction: That is, how the Liquid may rise in a small Tube, by the Attraction of the Parts thereof by the concave Surface of the Glass.

Let

Fig: 1.



Let $A B C D$ be a *small Tube*, *perpendicularly* immers'd in a *Liquid*, whose *horizontal Surface* is $E C D F$.

The *Parts* of the *Liquid* at $a a, b b$, adjoyning to the *concave Surface* of the *Tube*, are *strongly attracted* by it, and that in a *Direction perpendicular* to the *sides* of the *Cylindrick Glass*; or (which is all one) *parallel* to $E F$, the *Surface* of the *Liquid*.

Now the *Particles* $a a, b b$, *gravitating* in *Directions perpendicular* to $E F$, that is, *parallel* to $A C$ and $B D$, the *Sides* of the *Tube*; by means of the *afore-said Attraction*, it comes to pass that the *Particles* $a a, b b$, have all of 'em a much-less *Momentum* or *gravitating Force* than otherwise they would have, were the *Attraction away*. Therefore the *Parts* of the *Fluid*, which lie *immediately under* them, are *much less press'd upon*, than otherwise they would be.

And altho' the *Particles* $d d$ lie farther out towards the *middle* of the *Tube*, yet in a very minute and slender one (such as we here speak of) they are near enough to be within the reach of the *powerful Attraction* of the *Surface*, so far as to be in some measure influenc'd thereby; either *immediately* or *mediately*, by the means of the *Particles* $a a, b b$, which are *strongly urg'd* towards the *Glass*, and do (by the *General Law*) attract the neighbouring *Particles* $d d$, towards themselves.

Upon these accounts, the *Momenta* of all those *Particles*, comprehended within the *Circumference* of the *lower Basis* of the *Tube*, being much lessen'd; the *Fluid*, which lies directly under 'em, is proportionally less press'd.

But the Parts of the Fluid *ffff*, which lie between the Surface *E C D F* and the Bottom of the Tube, at more remote distances from the Sides of the Tube than its Semidiameter; These Particles (I say) being out of the reach of any such Attraction, do gravitate with their whole Force or Momentum on the Parts which lie under 'em. Therefore it appears, that by the Immersion of the small Tube into the Liquid, the Equilibrium is destroy'd between those Parts of the Liquid lying within the Circumference of the lower Basis, and those which are plac'd without. Therefore (by the Hydrostatical Laws) the Liquid must rise within the Surface of the Tube: For the stronger Fluid will still press in upon the weaker, and force it away before it. That is, the Particles about *aa*, *bb* principally; and next to them, the Particles about *dd* must necessarily give way to the Particles below them, which are urg'd on by the superiour Momentum of the Particles which come from the afore-said remote distances about *ffff*. From whence it follows, that those Particles about *aa*, *bb*, must necessarily ascend higher in the Tube, as to *eee*.

When they are risen higher, the Attraction to the Sides of the Tube will take place as before, and by lessening their Momenta, with respect to those below 'em, will give a new Occasion to the external Fluid, to insinuate it self within the Bottom of the little Tube, and consequently to push those Particles still up higher, as to *gggg*.

Thus, by the continued Action of the same Cause, the same Effect follows, and the Liquor continues to ascend in the Tube, till it comes to a certain determinate Height, where it keeps its station, and that by vertue of the same Laws which brought it thither.

And

And this, I hope, may be a satisfactory Account of the Reason of this Phenomenon; and serve to shew the Manner and Way by which Liquids may be conceiv'd to rise in small Tubes. I must say, *it appears to me to be the True one.*

NB. I spake but now (with a *Particular remark*) of the Pressure of those Particles of the Fluid, which lie at more remote distances from the Tube, than its *Semi-diameter*. 'Tis to the *Energy and Force* of these, that the Ascent of the Fluid seems chiefly, if not entirely, to be owing. For those Particles nearly adjoining to the *Convex* Surface, are attracted in some degree, as well as those which are approach'd to the *Concave*; And therefore can't be imagin'd to have any such *preponderating Momentum*, as to force those within to ascend in the Tube. But such Particles as are plac'd at farther distances beyond the *Convex* Surface, suffer no attraction from it, and so are sufficient to *press the Liquid away before them.*

But it may be said here, that if the surrounding parts of the Fluid *without*, on the *Convex* side, were attracted as well as those *within*, on the *Concave*; then the Fluid ought (by these Principles) to rise *without*, on the *Convex* surface of the Tube, as well as it does *within*, on the *Concave*: which Experience shews that it does not.

But (in answer to this) the Reason why it ought not to do so (or at least very inconsiderably) is obvious, I think, from the different natures of *Convexity and Concavity*. Suppose we a small Particle of a Fluid, in contact with the *Convex* Surface of a Tube: 'Tis plain, that all the *Lincolns* or *streight Filaments* composing this Surface, are averted, or turn'd from the aforesaid Particle, except that one single Filament,

in which it touches the Surface. But on the *Concave* side, all the Filaments are turn'd towards such a Particle, which we imagine to be in contact with the Surface there.

From whence arises an exceeding great Difference between the Attractions of a Particle of Fluid Matter, by the Filaments on the *Convex*, and on the *Concave* side.

For in the *Former* Case, the said Particle must be vastly more out of the reach of these Attractions, upon the account of the averted Position of the Filaments, than in the *Latter* Case, where the *Incurvation* turns the attracting Lineole towards it, and by that means presenting a much-greater Force, produces a proportionally-greater Effect.

And for this reason, the Parts of the Fluid within, losing beyond comparison more of their Momentum than those without do; the Fluid ought to rise beyond comparison more on the *Concave*, than on the *Convex* Surface of the Tube. That is, its ascent on the outside ought to be scarce sensible; and I believe it will be always found to be so.

Thus far we have shewn the Reason why the Liquor must rise in the *small* Tubes.

From hence now it follows likewise, that it must of necessity rise higher in Tubes of a smaller, than those of a larger Bore.

For suppose there be two Forces, each of which is to lift a several Weight. Now (from the Principles of Mechanicks) that Force which bears the greatest Proportion to its Weight, will be able to raise that Weight higher, than that Force which bears a less Proportion to its Weight, will be able to raise that Weight. Let us also take two Tubes of different diameters, and the same height :

height: the attractive Powers are as the Surfaces; and the Weights or Quantities of Liquor to be rais'd into these two Cylinders, are as their solid Contents. Or (because the Heights being the same, the Surfaces are as the Peripheries; and the Contents, as the Areas of the Bases;) the Attractive Forces will be as the Peripheries; and the Weights, as the Areas of the Bases.

But there is a greater Proportion between the *Peripherie* and the *Area* of the *Base* in the *small Cylinder*, than there is between the like quantities in the *great one*. Therefore in the *small Tube*, the *Attractive Force* bears a greater Proportion to the *Weight of the Liquid to be rais'd*, than it does in the *great one*: And therefore the *Liquid must rise higher* in the *Former*, than in the *Later*.

From hence likewise we may form a *Rule*, that may help towards determining the *Height* the *Liquid must ascend to*, in any given *small Tube*.

For the *Liquid must necessarily rise*, till it comes to such a *Height*; that the *Momentum of all the Liquid in the Tube*, as it is there diminish'd by the *Attraction of the Surface*, becomes equal to the undiminish'd *Momentum of the External Liquid*, at that depth the *Tube* is immers'd to. And when it is come to that particular *Height*, it must as necessarily stand, and go no farther.

And in that Case, the Proportion will run thus—As the *Diminish'd Gravity of the Liquid in the Tube* is to the *Absolute Gravity of the Collateral Cylinder of External Liquid*, so is the *Depth of Immersion*, to the *Height of the Liquid in the small Tube*. For, I suppose the *Cylinder of Fluid in the Tube* to be balanc'd by one without, which has the same *Base*, and whose *Height* is equal to the *Immersion*; for the *Bases being the same*, the *Heights are as the Contents or*

Quan-

Quantities of Matter. And to make an *Equilibrium*, or *Equality of Momenta*, the *Forces* must be *reciprocally*, as the *Bulks* or *Quantities*; that is (in this case) *reciprocally* as the *Heights*.

Now, as for the *Reasons* inducing me to propose a *Solution* of the *Phenomena* of *Capillary Tubes*, on such *Principles* as I have here done; I think it not improper here to subjoin them.

'Tis true, the *direct* and *seemingly-straight* ascent of the *Liquid*, from the *lower* to the *upper* parts of the *Tube*, would, at first view, tempt one to think of no more than only an *Attraction upwards*, or in *Directions parallel* to the *sides* of the *Tube*, instead of *perpendicular* thereto; and so to derive the *Fluid's* ascent only from this, without any regard to *Hydrostatical Laws*, or the *loss and recovery* of an *Equilibrium*. But in this way there appear'd several *Difficulties*, of that strength, that I could by no means get over them; and the consideration of these, determin'd me to solve the *Phenomenon* the other way.

For, 1. I could not see any *Reason* to convince myself, why a *Particle* of the *Liquid*, as *a* or *b*, which is there at that point in *actual contact* with the *Glass*, should not be attracted to and by that *Particle*, rather than by another above it, and remote from it: Or at least why it should not be *vastly more* attracted by that *Particle* with which it is in *contact*, than by another above it; and consequently why the *Attraction* should not *first begin*, in *lines perpendicular*, and not *parallel*, to the *sides* of the *Tube*, whatever be done afterwards: For I do not absolutely exclude the *former Attraction* from being some way concern'd, tho' I make the *latter* the *main* and *principal Cause* of the *Ascent*.

2. If a Body resting on an *Horizontal Plane* were to be drawn (in a Direction parallel thereto) against an *Upright or Vertical Plane* erected upon the same *Horizontal Plane*; it appear'd certain to me, that the *actual Pressure* of that Body in the *Horizontal Plane* would be diminish'd, according to the force wherewith the *Thread* was drawn, which press'd it against the *vertical Plane*. For it may be drawn so hard against the upright Plane, that the *Horizontal* one shall have little or nothing at all of the burden of it. And applying this to the Case in hand; I concluded, that for this reason the Momentum of the Parts of the Fluid *aa, bb*, must be abated, with respect to the Particles lying immediately under them.

3. When I suppos'd the Liquor to ascend purely by virtue of the Attraction directly upwards, in Lines parallel to the sides of the Tube, I could not see a reason, why the Liquid should ever stop in any *Capillary Tube* before it comes to the very top of all; which yet Experience shews it does. For if the Attraction be solely from the upper parts, then as long as there is any part of the Surface left unoccupied by the Liquid, so long there is a Cause left in being, of the Liquids farther ascent. And if there be that Cause in being, why should it not exert it self, and make the Liquid rise, as well when 'tis gotten up 2 or 3 inches, high in the Tube, as when it was below at the bottom? All Circumstances here, are the same as there, as far as I can with all my Attention discover. And that the Liquid has already possess'd some part of the Surface, can be no Reason why that part which is not possess'd, should not exert its attractive force, and draw up the Liquid, till it is gone as high as it can go; that is, till the whole Tube is full. In a word,

word, Why should I deny the *upper part* of the Tube that attractive power, which I so freely allow to the *lower*? I know no reason, to imagine this Vertue to lie so *unequally scatter'd about the Tube*; and if it be not so, then I think I ought to expect the same Effect should take place in the upper parts of it, that does in all other places.

And therefore, 4. By the Explication which I have here given of this Phenomenon, I found I could give a Reason for the *Ascent of the Fluid to a determinate Height in any Capillary Tube propos'd*; which I saw no way, that was tolerably Philosophical, to do by the other Method.

For the *Momentum of the external Liquid* being in some measure taken into this Account, as well as the *Attraction of the Tube*; the *Hydrostratical Laws*, by which I suppose the Liquids *within and without* to be *balanc'd*, will themselves determine the Height to which the Fluid must rise in the Tube. Otherwise (as I hinted before) 'tis not a slight Difficulty, to conceive any other Limits to bound the motion of the Liquor upwards, but the very top of the Tube it self: and there indeed it must stand for a good Reason. But if it settles at any determinate depth below that, and this by vertue of the *direct or upright Attractions*, I must needs say 'tis a Riddle, out of the Intricacies of which I have little hope to deliver my self.

For the remaining part of the *Concave Surface* lying above the Liquor in the Tube; either has, or has not, the Power of Attraction, like the other parts of the Surface below.

If not, by what Law is a Tube so divided into *attractive and non-attractive Segments*? How is the Limit between these two very Heterogeneous Parts determin'd?

Was

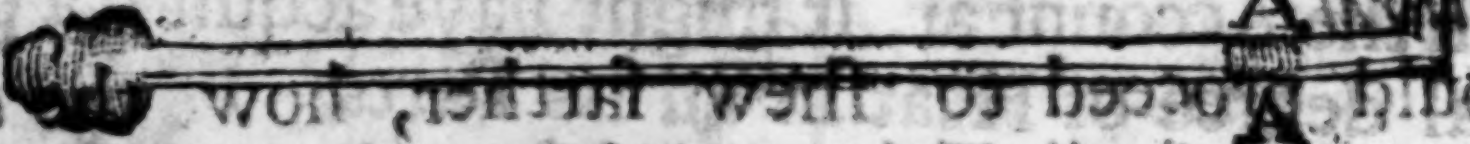
Was there *originally* and *always* such a distinction? Or did it commence when the Glass was first *blown into the Form* of a Capillary Tube? Or did it begin to take place only when the Tube was actually *immers'd* in the Liquid? In short, does this strange property owe its rule to *Nature*, or to the *Fire*; the *Blast* which made it a Tube, or to the *Water* 'tis *plung'd* in, when the Experiment is to be perform'd? If it be not to one of these, I am at a *loss* for its Original. On the other hand; if the remaining part of the Tube, *above the Surface* of the Liquor where it settles, be endow'd with an attractive force *impartly and proportionally* to the rest, why does it not exert it *itself*, when the Liquor is *fairly* presented *in the Sphere* of it; in like manner as the attraction of the other parts below did, when the Fluid was brought within their Sphere?

By how much the greater a Difficulty it will be, to answer these Considerations upon Just and *Philosophick* Principles; by so much the more (I hope) will the former Solution I have given, appear to be *clear* and *natural*.

I could proceed to shew farther, how the other Phenomena of small Tubes might very naturally be solv'd from these Principles; but it being very easie to make their Application, I shall now turn to the Dissection of those matters, and with that I have now said, conclude this present Subject.

An Account of an Experiment concerning the different Densities of the Air, from the Greatest degree of Heat, to that of Cold, in our Climate.

I Took a Glass Tube about 2 Foot long, and of about $\frac{1}{2}$ inch diameter, which I bent into the form of a Rectangular Syphon, at nearly the distance of 6 inches from one end, thus:



At that Extremity *which was farthest from the Angle*, I cemented on a Brass Screw with a small Perforation in it; by which means, when I had put a little Quicksilver in at the shorter Leg, by inclining the longer Leg *this or that way*, I could bring the Mercury to rest any where at pleasure; as at A A. The little Column of Mercury I here made use of, was about $\frac{1}{2}$ inch in length, as being sufficient for the purpose of this Experiment. Then having screw'd a Cap on to the longer

longer or Horizontal Leg of the Syphon, and the Mercury being fix'd and settl'd in a certain part; I convey'd it into a Trough, together with a Thermometer, and pour'd on as much warm Water as would cover the Ball of the Thermometer. Thus the Syphon lay, with its longer Leg under the Water, in an exact Horizontal Position, and its shorter Leg upright and above the Water. And the Result of this Contrivance, was the Prevention of some Inconveniencies, which would have follow'd upon placing the Syphon so, as that the Water should have found its way into it. Besides, the Action and Power of the External Air could not have been so certainly argued and determin'd, in that case, as it might in this, where it had liberty to press as it would, and that immediately, without opposition or hindrance from an Intervening Body. The Spirit in the Thermometer, being quickly influenc'd by the warm Water, I suffer'd it to rise up as high as the little Ball on the Top of that Instrument, and indeed to pass into it; that so I might make my Observations on its Descent with more exactness. For I imagin'd, that by that time the Spirit was fallen to some convenient degree, design'd to begin the Account at, it might have acquir'd a pretty equal degree of Heat in all its Parts.

Accordingly I began my Observations, when it had descended to 130 Degrees above the Freezing Point; at which time I found the length of the Column of Air, from the clos'd end of the Syphon to the nearest Surface of the Quicksilver, to be just 144 tenths of Inches. After the Spirit had descended 10 Degrees lower, the Air, which before possess'd 144 Parts, lack'd one of them now; and so on successively at every 10 Degrees descent of the Spirit, the Column of the contain'd Air was

Z 2

lessen'd

lessen'd in its length one exact tenth. When it had descended to 30 Degrees above the Freezing Point, the Air was found to possess but 134 of the forementioned Parts. So that from hence it will be easie to conclude, that at the Freezing Point, the Air in the Syphon would be reduc'd to 3 tenths less than at the last Observation; and consequently at 50 Degrees below the Freezing Point, (which I am inform'd is the greatest Degree of Cold that has happen'd in our Climate,) it would be reduc'd to 126 Parts of the whole, and in that state would be one eighth more dense, than when at the greatest Degree of our Natural Heat. And the Reason why I could not prove this later part by Experiment, was, that when I came to expose the Thermometer and Syphon in the open Air, or freezing Mixture, the Syphon would instantly receive the Impression of the Cold, and the Air contain'd in it be considerably contracted, before the Thermometer gave any sign of such Alteration. But seeing the former part of the Experiment succeeded so very regularly as it did, I think there can be no doubt of the truth of the whole Calculation; nor do I yet see how it could be better perform'd. I shall add a Table of the different Degrees of the Air's Density at every 10 Degrees, from 130 above the Freezing Point, to 50 Degrees below it.

This Experiment was made February the 11th, 1708, the Mercury in the Barometer (at the same time) standing at 30 inches.

Degrees

Degrees.		Parts.
Above	130	1445
	120	143
	110	142
	100	141
	90	140
	80	139
	70	138
	60	137
	50	136
	40	135
Freezing Point.	30	134
	20	133
	10	132
	0	131
	10	130
	20	129
	30	128
	40	127
	50	126
		125
Below		124
		123
		122
		121
		120
		119
		118
		117
		116
		115

This Table shews the Difference of the Air's Density at every 10 Degrees, from 130 above the Freezing Point, to 50 below it. As, supposing the Spirit in the Thermometer should stand at 40 Degrees above the Freezing Point; I find right against it, in the third Column 127. Its State being then so much more dense than when the Spirit is elevated to 130 Degrees. And so of all the rest.

The second Column shews the Extent of the Air at the several Stations, from the Greatest Heat, to the Greatest Cold.

NB.

NB. For the better understanding of the foregoing Experiment, I would note something concerning the *Position and Motion of the little Column of Mercury in the Horizontal Leg of the Syphon.*

This body of Quicksilver being fix'd in a certain part of the Tube (as suppose *thereabouts*, where it *now appears* to be, in the *Fig.*) was afterwards, by the Rarefaction of the Air (contain'd between it, and the end cover'd with the Cap) driven farther towards the Angle of the Syphon; for the Mercury when put in, necessarily forcing the Air along before it, there must needs be a Column of Air, included between it and the end of the Syphon guarded with the brass Cap; and that Air must as necessarily be rarefied by the heat of the warm Water, and that Rarefaction or Expansion will force the Mercury towards the Angle of the Syphon, where it has only the Pressure of the External Air (thro' the open shorter Leg of the Syphon) to encounter with, as an Impediment to its motion that way. Now the first Expansions of the included Air, by the Heat, are sufficient to overcome the contrary Pressure of the External Atmosphere: And by this means the Column of Mercury is press'd towards the Angle of the Syphon so far, till the Rarefaction and the outward Pressure come to balance one another. Then, as the Water cools, (and the Heat growing less, the expansive Force of the included Air by consequence abates too) the Pressure of the Atmosphere thro' the shorter Leg of the Syphon begins to prevail, and consequently forces the Mercury more inwards, or farther from the Angle of the Syphon. And thus the Rarefaction still diminishing, and the weight of the Atmosphere gaining more and more upon it, the Mercury is still driven farther from the Angle of the Syphon, and so the length
or

or distance between it and the end cover'd with the Cap, becomes continually less and less. And these Distances are express'd in that Column of Parts in the Table, which answer to the Degrees of the Spirit's descent in the Thermometer.

This Experiment it self proves the Spring of the Air, as it shews us the External Atmospheric Column first giving way to the more powerful Expansion of the included Air, and then by degrees recovering it self, and forcing the Mercury away before it, towards the other end of the Tube.

We see likewise the Ground gotten by the External (which is the same with that lost by the included Air) to answer exactly to the Abatements of Heat indicated by the descending Spirits in the Thermometer; so that the Liquor there always gave an exact and perfect account of the contraction or shortning of the Column of included Air; that is, of its Density.

The abundant Usefulness of which Observation, I may some time or other more largely discourse of.

EXPERIMENTS

Concerning the Refraction of the Air.

ABOUT Ten Years since, that Curious and Ingenious Member of the ROYAL SOCIETY, Mr. JOHN LOWTHORP, contriv'd an Apparatus to demonstrate sensibly the Refractions of the Air, which hitherto had been perceiv'd only by the subtle and nice Divisions of Astronomical Instruments. He made a Vacuum between two inclined Planes of Glass by

by the help of *Quicksilver*; thro' which an Object view'd with a *Telescope* was seen, upon readmission of the Air, very sensibly to change place: An Account of which Experiment is at large in *Phil. Trans.* N^o 257, and in the *French Memoirs* for the Year 1709, to which I referr.

Mr. *Cassini* the Son having been present when Mr. *Louthorp* made his Experiment before the *Royal Society*, made a Report thereof to the *Royal Academy of Sciences of France*; and, upon his return home, those *Scavans* thought it worth their while to re-examin the matter: But tho' themselves thought it very strange, yet, as they manag'd the Trial, they declare in the History of their Academy for the Year 1700, That it did not succeed; and, that the Beams of Light passing thro' such a *Vacuum*, suffer'd no alteration by Refraction.

How well they made their *Vacuum*, tho' they say it was *bien exactement*, may justly be question'd; or rather, the thing being so evident, it will not seem malicious, if we suppose some little Sense of Emulation might incline them to deny the Honour of an Improvement of such Consequence to *Astronomy*, to a foreign Academy.

The *Royal Society* (whose Glory it is to be as unwilling to deceive as to be deceived) being inform'd that this Experiment was call'd in question by the *French Academy*, were desirous that it might be put past dispute, by repeated and fully-attested Trials: Accordingly I was order'd to make an Instrument for the purpose, by the direction of Mr. *Halley*, R. S. S. and Professor of Geometry in *Oxford*. It consisted in a strong Prism of Brass, two sides of which had Sockets, to receive Glasses as truly plane and polish'd as could be gotten; and the third side had a Pipe with

with a Stop-cock, whereby to apply both the Exhausting and Condensing Engine. The Glasses were firmly fix'd and cemented in, so as to bear both an inward and outward Pressure, and the whole turn'd upon an Axis, that it might be made to receive the Rays with any Obliquity desir'd: And, to be the more secure, I affix'd a Mercurial Gage, to discover any the least Defect in the Cement, that might happen; the Angle contain'd between the two Glass Planes being very near to 64 degrees. And this Instrument, thus prepar'd, we fitted to a Telescope of about 10 foot long, so as the Axis of the Telescope might pass thro' the middle of the Prism; and in the *Focus* of the Telescope a very fine Hair was adapted, for direction to the Sight.

Having chosen a proper and very distinct erect Object, whose distance was 2588 feet, *June 15. S.V. 1708*, in the Morning, (the *Barometer* being then at 29.7 $\frac{1}{2}$, and the *Thermometer* at 60) we first exhausted the Prism, and then applying it to the Telescope, the horizontal Hair in the *Focus* cover'd a Mark on our Object distinctly seen thro' the *Vacuum*, the two Glasses being equally inclin'd to the visual Ray: Then admitting the Air into the Prism, the Object was seen to rise above the Hair gradually, as the Air entred, and in the end the Hair was found to hide a Mark 10 $\frac{1}{4}$ inches below the former Mark. This often repeated, as often succeeded.

This done, we applied the Condensing Engine to the Prism, and having pump'd in another Atmosphere, so that the Density of the included Air was, by the Mercurial Gage, double to that of the outward; we again plac'd it before the Telescope, and then letting out the Air by the Cock, the Object,

A a

which

which before seem'd to rise, now appear'd gradually to descend, and the Hair at length rested on an Object higher than before by the same Interval of $10\frac{1}{4}$ inches. And this likewise often repeated, never fail'd.

We again crouded in another Atmosphere, and upon discharging the condens'd Air, the Object was seen near 21 inches lower than the Hair; but in this, the great Pressure forcing the Cement would not permit us to make so frequent Repetitions as in the former.

And these Experiments have been shewn before the President; and, at times, to most of the principal Members of the *Royal Society*. So that 'tis hoped the Fact may no longer be question'd.

Now the *Radius* being 2588 feet, ten inches and a quarter subtend an Angle of one minute and eight seconds; and the Incidence of the Visual Ray being 32 Degrees, by reason the Angle of the Glass Planes was 64 Degrees, it follows, from the known Laws of Refraction, that as Sine of 32° to S. of $31^{\circ}.59'.26''$, so Sine of any other Incidence to the Sine of its refracted Angle; and so is *Radius*, or 1000000, to 999736; the Logarithm of which *Ratio* is —0001145: whence the Refraction of the Air may readily be computed at any other Angle of Incidence.

By these Experiments it plainly appear'd, that the Refraction of the Air was, as far as the Eye could distinguish it, exactly proportion'd to its Density; the Refraction being the same from the common Air to a *Vacuum*, as from a double Density to the com-

common Air, and the Refraction from a treble Density to the common Air exactly double to that from the common Air to a *Vacuum*. Whence the Density of the Air, in respect of the incumbent Atmosphere, being always as the height of the Mercury in the Barometer, the Refraction also will be *ceteris paribus* in the direct proportion of the heights of the Mercury.

But this Density of the lower Air is considerably varied by Heat and Cold, as appears by the Table, in page 173; wherein we have shewn by Experiment, that the same Air which when the Thermometer marked 130 degrees, (being the greatest Summer Heat) occupied 144 spaces, by extremity of Cold, or at 50 degrees below the freezing point, was reduced to 126 of the same spaces; but at the freezing point to 131: it being very remarkable that the Air and Spirit of Wine did proportionable contract themselves during the whole Experiment. Hence by help of the aforesaid Table, we are enabled to give a rule to estimate the Refraction of the Air at all times; having the height of the Barometer and Thermometer: for with the same Heat, the Refraction is as the height of the Barometer directly, and under the same Pressure, it is as the spaces the same Air occupies reciprocally.

Now our Experiment being made when the Mercury was at 29, $7\frac{1}{2}$, and the Thermometer at 60, which gives the space in the Table 137; let it, for Example sake, be required to find what would be the Refraction when the Barometer is at 29 inches, and the Thermometer at the Freezing point, or the Air occupying but 131 parts. I say, the Density of the Air at such time will be to the Density at the time of our Observation, as 137 times 29 to 131 times 29, $7\frac{1}{2}$,

that is as 15892 to 15589; wherefore the Refraction of the Air, at such time, will be in that same Ratio to what it was June 15, 1708. How to apply this rule to Astronomical purposes, and how to correct the Errors occasioned by the Air's Refraction, in the Observations of the Stars, will shortly be set forth in a more proper place.

An Account of an Experiment concerning the different Weights of the same sorts of Bodies, but of very unequal Surfaces, in Water, which were of equal Weight in Common Air.

IT is very well known, by many Experiments, that the minute Parts of Bodies, which are specifically heavier than some Menstruums, may, notwithstanding their excess of Gravity, be suspended and held up therein.

This is seen in the Dissolution of Gold in *Aqua Regia*, and of Silver in *Aqua Fortis*, and many other Chymical Experiments besides.

Now these Phenomena have hitherto been us'd to be solv'd from the consideration of the great encrease of Surfaces (in small bodies) in proportion to their bulk. For these Metals, or other Bodies, (say those who go upon this Hypothesis) being divided into extreemly-minute parts, by the action of the Menstrua, a vast encrease of Surfaces, in proportion to the bulk or weight, is an immediate Consequent thereupon. And

And the Resistance from the Liquid, being greater or less, according to the Superficies, it comes to pass that Particles of Matter, specifically heavier than a Fluid propos'd, may by that great excess of *resistance* above their *gravity*, come to be *suspended* and *float* therein.

Now from hence 'twas easie to infer, that if this was the reason of the Phenomenon, something of this mighty difference must needs appear by *weighing equal quantities of Matter*, and therefore *equally heavy*, but of very *unequal Superficies*, in *Water*, or some other *Liquid*; and then seeing how much the one exceeded the other in weight there. Accordingly I took a piece of *Sheet-Brass* of an exact *inch square*, and in weight just 482 grains. I then cut as many *Square inches* of *Brass Tinsel*, as were equal in *weight* to the former, *viz.* 482 grains; and these pieces were 255 in *number*. Now here being so very great a difference of Superficies, I concluded there would be some very considerable difference found, arising upon the weighing of these Materials in *Water*. But to my great surprize (being indeed prepossess'd in *favour of the common Opinion*) I found but two grains difference; the single piece weighing in the *Water* about 422 grains, and the other separate ones hardly two grains less. And this, upon two or three repeated Trials, (made withall the caution imaginable) succeeded much the same; so that the difference is not worth mentioning. Now here the proportions of the Surfaces were as 1 to 255 (for I reckon the sides of all the *Tinsel-Lamina* to be equal to the sides of the *single Brass-Lamina*) and notwithstanding that in one case there was 254 times more Superficies, than in the other; yet there was scarce

a $\frac{1}{11}$ part *less weight* with all that Superficies, than with the former; which decreement of Weight may, without scruple, be attributed to some small Bubbles of Air, which adhered to them unperceiv'd.

And from hence I am strongly induc'd to conclude, that some other Cause must be found out to solve this Phenomenon by, since the disproportion between Superficies, and Bulk or Weight of Matter is not sufficient to do it. For suppose a small *Metallick* Particle, or one of some *other* Body *specifically heavier* than a *Liquid*; and suppose this to sink by its own weight, if put into this Liquid. According to the foregoing Experiment, tho' this Particle were *divided* so, as to have 254 *times more Superficies* than now it has, yet its loss of Weight would be so inconsiderable, that no *suspension in the Liquid* were to be expected from thence. And there is a pretty remarkable Confirmation of this, to be drawn from an *Experiment* I made with the Powder of *Fine Flint* Glass. This Glass made use of, was of that sort which is of all others the clearest and *freest* of *Blebs*. Farther; to have the parts of the Glass as *minute* as well might be, after it was reduc'd to Powder, I pass'd it thro' a *Lawn Sieve*. And that there might be no Errour arising from the want of a *just Quantity of Matter*, to make the Trial with; I weigh'd an Ounce of this fine Powder against the like quantity of *solid Glass*.

And here likewise (as in the former Experiment) the weight of this so finely-powder'd Glass, in *Water*, differ'd by *such a Trifle*, from the Counterbalance of the solid Piece in the *same Element*, that it was by no means worth taking notice of: Especially too, since some parts of it *remain'd floating* in the Water, and never set-

settled, or sunk down into the *Bucket* of the *Hydrostatical* Balance at all.

But what this way of arguing from the greatness of the Surfaces of Bodies, and the *resistance* by the *Liquid* arising therefrom, will not do (with respect to the accounting for that Suspension in a *specifically-lighter* Medium) I believe may be done by another Method, and that *effectually*. In short, the suspension of the heavier Particles of Matter in Liquids, I attribute to the *same Cause that keeps the Liquors suspended in small Tubes*: I mean *Attraction*.

The *minute* Parts of Bodies consisting of plane Surfaces, being strongly attracted by the Parts of a Fluid, in which they are plac'd, (and therefore reciprocally attracting the Parts of that Fluid again) may, by the *Action* of these Forces, be held suspended therein. And what little Bodies are not, or will not be, held suspended in a Liquid, but are let fall therein to the bottom of the containing Vessel, I believe to be so, upon one of these two accounts: Either that the Parts of the Liquid *do more strongly attract one-another, than they do those little Bodies interspers'd amongst them* (which therefore *subside* upon that score;) or else, that they do by their own *Attractions* form themselves into little clusters, whose bulk and *superiour Momentum* help to precipitate them downwards. This being laid down as the *true Cause*, of the *Suspension* of small ponderous Particles of Matter in Liquids; I believe our common Notions of *Corrosion* and *Dissolution*, may also be rectify'd from the same Principles. A *Corrosive* Liquor or *Dissolvent*, in the vulgar sense, is a very *unintelligible* thing. For (not to mention other *Incongruities*) 'tis not to be conceiv'd what should carry the parts of a Liquid with so prodigious an *Impetus* into the Pores of a solid Body, so as to dissolve the whole Texture.

ture of it, and reduce it into *insensibly-small* Parts. But an *Attractive Force* in that solid Body will do this; by which the Particles of the Fluid are forc'd into the Interstices of it, with a *Momentum superiour to that of the Cohesion of its Parts*. For this suppos'd; its Parts will be separated from one-another; that is, the Body will be *dissolv'd*. But the time perhaps will come, when this *wonderful Law of Attraction* (as it obtains in the smaller Portions of Matter) will be more fully and clearly understood, and some new Effects of it discover'd, which now are not suspected to proceed from that Cause.

AN

APPENDIX,

Containing some General Remarks on some of the foregoing Experiments.

THO' there are none of the Experiments related in the *foregoing Tract*, but what will (I hope) be of some use to the *Intelligent Philosophical Reader*, (serving at least to excite him to make farther Improvements himself, in *Experimental Knowledge*, if they don't give him all the Information he needs or desires) yet there are some of them which being (I think) *quite new*, and moreover *very surprizing*, I thought it might not be amiss to fill up a few Pages here with some Enquiries into, and Reasonings upon them.

The Experiments I *principally* referr to, are those of *Electricity* and *Light* produc'd by *Attrition*; of which the Reader may find a large account from *pag. 17. to pag. 69.* and these relating to *various sorts* of Bodies, and in various *Mediums* too.

I begin with the Phenomena of *Electricity*.

There are some of these so strange in their Circumstances, that I confess I am apt to think there are *not many in nature*, more surprizing then they are.

But, tho' the discovery is yet but *young*, and has not been made long enough, to be *thoroughly and perfectly discuss'd*; yet some things which are either *plain and certain*, or *probable and likely*, may be

advanc'd in the mean time ; and as all Circumstances and Varieties in those Experiments come to be more accurately examin'd, 'tis to be hop'd we may arrive at more *Positive Conclusions*, about the *Reasons of the Phenomena*.

The Four following Propositions relate to the Attrition of Tubes.

Prop. 1. *Within the Body of the Glass, are contain'd and lodg'd certain Parts of Matter, of considerable Force and Activity, which by their Motions and Percussions are the Causes of all these Effects.*

That there is an emission of some Matter consequent on the Friction, I think is too plain to be question'd; for 'tis obvious almost to every one of our Senses: To the Eye; by the Motions of the Leaf-Brass, and by the Light produc'd, when the Tube was rubb'd in the Dark: To the Feeling; by the sensible strokes and pushes, made upon the Face, when the Tube was held near it: To the Ear; by the Noise and Crackings, the Eruption was accompanied with, which might be heard at the distance of Seven or Eight Foot.

That this Matter emitted, is also emitted from or by the Tube; I take to be as plain as the former. For how else should the rubbing of the Tube, ever be an occasion of this Matter's displaying and exerting it self? If it came not from thence, the Attrition of the Tube could not fetch it from any other Body distinct from the Tube. But the Testimony of Sense assures us of this likewise: For all the motions of the Leaf-Brass are directed to, or from, or about the Tube; and therefore tis beyond all dispute, that the course of the moving Matter is from thence. And

I believe there's hardly any one but will allow, that this Matter, if it came from the Tube, was certainly re-pos'd and lodg'd there before.

Prop. 2. *The Motion of this Matter is not equable and regular, but disorderly, fluctuating and irregular.*

This appears from part of the Fact related. For the little Bodies sometimes would be drawn to, sometimes thrown from the Tube with violence; sometimes be suspended for a small time in the Air, and at other times slip along the sides of the Tube. They would repeat these Leaps and Boundings for several times together, and flutter up and down almost like so many Animals, rather than pieces of lifeless Matter. Now this Variety cannot be the Effect of an even and regular Motion. It plainly shews the moving Force to exert it self (as it were) by fits; and to be propagated every way about in a confus'd irregular Orb. For if Bodies once put in motion, can't of themselves alter their Direction, but are overrul'd by a foreign Force whenever they do it; and if the diversities of their Motions must needs infer just as great a diversity in the Impulses of the Bodies that move them; then since our pieces of Leaf-Brafs (in this Experiment) were so very odd and extravagant in their Motions, 'tis plain, that the Effluvia (which alone can be the moving Bodies here,) must themselves also be hurried after a very irregular manner.

Prop. 3. *The Air contiguous to the inner Surface of the hollow Tube, has an Influence on Operations of the Effluvia.* This plainly follows, because when the Tube was exhausted, and the contain'd Air drawn out, the Leaf-Brafs would scarce be stirr'd at all,

tho' with a much more forcible Attrition, and at a much less distance, then when the Tube was full of Air. And besides, when the Air was let into the Tube again; the attractive power (which was before almost lost) was strangely and suddenly recover'd again. Which is an undeniable Proof, that the Presence of that contiguous Air did some way or other contribute to the more powerful and effectual operation of the Effluvia. Neither is it an objection of any moment against this; that the attraction is as powerful in the *case of the solid Tube*; where there being no *Cavity*, there can consequently be no *contiguous Air*. For this only proves that there is as strong an attraction in a solid Tube, as in a hollow one; but it does not prove, that the Air was of no advantage in the case of the hollow Tube. *To prove that an Effect may be the same, in two very different Circumstances; is not the same thing as to prove that it has no manner of relation to this or that particular Cause in one of those Circumstances.* And therefore to argue from the Leaf-Brass being stirr'd as vigorously by the Effluvia, when the solid Tube was used; will not be sufficient to show that the Air has no manner of influence in the Circumstance of the hollow Tube.

For the Proposition does not assert, that the Effluvia can in no case exert themselves with vigour, without the concurrent assistance of the Air: but it asserts, *that the contiguous Air had some advantageous Influence in the Case of the hollow Tube.* And this is as evident, as that the Air is serviceable to the vital Functions of Animals, or that they cannot live and breath without it. For as upon the depriving an Animal of the benefit of this Element, *all the Powers flag; the Springs of motion become feeble*
and

and drooping; and at last sink away into a fatal inactivity: So here, if the Tube be exhausted of Air, the Effluvia loose all that briskness which wrought such surprising Effects before; and continue (as it were) impotent and dead, till a fresh return of Air inspires them again.

And to add a Demonstration of the Airs power, with respect to the operation of the Effluvia, which will (I think) admit of no Exception; I desire it may be consider'd, that the Effluvia will not be excited by any Friction, to produce any Effects, if the Attrition of the Tube be made in Vacuo: and that, whether it be a closed hollow Tube replete with Air, or even a solid Tube it self: To either of which, I can give what degree of Friction soever is necessary, in an exhausted Receiver. The contiguous Air, I say, being removed, the Electrical force seem'd to be quite gone; and continued so to be, till the presence of the Air was restor'd. Now this is a plain Proof of the necessity of the Air, to the operations of this attractive matter. Wherein that necessity lies, or what assistance tis which the Air contribut's, I don't here determine; but that the thing is so, is so manifest, that I cannot expect to see any thing more clearly prov'd by Experiment than this is.

Prop. 4. It does not seem that the Air included in the Cavity of the Tube can have any Influence (with respect to the Action of the Effluvia) but one of these two ways: Either, by the forcible Endeavour of its Spring against the contiguous Body of Glass, helping to push and impell that active Matter outwards, which is already prepar'd and dispos'd by the Attrition for such a Remove; or else, as (by vertue of the same Principle) it hinders the Electrical Matter from retiring
in-

inwards, by acting as an Impediment against it; and so only occasionally causes the more sensible and remarkable Effects of that Matter, upon little Bodies plac'd in its way without the Tube.

It may be, that the Heat produc'd by the Motion and vigorous Attrition of the Glass, may produce some degree of Rarefaction in the Air contiguous to the convex or outward Superficies. And then, in that Case, there being not the like Rarefaction in the Air contiguous to the concave or inner Surface, (for the rubbing cannot produce that Heat upon a distant Surface, that it does on that which is immediately rubb'd;) the Electrical Matter will, with much more difficulty, retire in towards the Cavity of the Tube, than it will go outwards: because the Equilibrium being lost on the outside, it will necessarily be carried that way where it meets with the least Opposition. And certainly, the Spring of the less-rarefied Air within, is superior to the Pressure of the more-rarefied Air without.

And therefore, on the other hand, when the Tube is exhausted of its Air, and consequently the Balance lost on the inside; all the Attrition that can be given will not be sufficient to bring the Effluvia out against an incumbent Pressure, as long as the inward Cavity is clear of Air, and there is no Counter-force to oppose their Conatus or Tendency that way.

Prop. 5. As the internal Air is necessary to the Action of the Effluvia, so is the external too: Because, tho' the Tube were full of Air, yet if rubb'd in Vacuo, the attractive Power was quite lost.

Prop. 6. As therefore the internal Air seems necessary, either to assist the Electrical Matter in its Motion outwards, or at least to prevent its retiring inwards; so the external
Air

Air appears to be as necessary to carry the little Bodies (which we say are attracted) towards the Tube.

For if by the Heat and Rarefaction, consequent upon the Attrition, the Medium contiguous to the Tube be made specifically lighter; then of course, to keep up the balance, the remoter Air, which is denser, must press in towards the Tube, and so carry away (in the Torrent) the little Bodies lying in its way, thither also.

Prop. 7. The various Irregularities in the excitation, or the emission and discharge, of the Electrical Matter from the Tube (which will be follow'd with proportional Irregularities, in the Motion and Tendency of the denser Air, towards the Tube, by the Hydrostatical Laws) may be sufficient to account for the various uncertain Motions of the little Bodies carried towards the Tube.

I shall now add something concerning the Effects of the Electricity of the Glass Globe and Cylinder.

Prop. 1. The Presence of the Air is necessary to this Phenomenon, of the regular Direction of the Threads; as well as to that of the Attraction of the Tube.

Because, if the semi-circular Hoop of Threads were plac'd in *Vacuo*, that Property of their regular Direction to a Center would be quite lost, even tho' the Globe or Cylinder were full of Air.

Prop. 2. The reason therefore, why the Threads are not directed in this case, does not seem to be, because there is no Electrical Matter discharg'd from the Glass (by the Attrition) to draw and direct them thither; but because there wants a current of External Air, to put them into the afore said central Direction.

For the external Air being absent, and the internal present; the Matter should find a vastly-easier passage outwards.

wards, than *Inwards*; and therefore ought to be discharg'd that way. But then, because the external Air is remov'd, there is *no room for the loss and recovery of an Equilibrium* to take place; and consequently no *Flux of a Circumjacent Medium* that way, and so, no *Direction of the Threads*. For,

Prop. 3. *If the Electrical Matter be emitted in Physical Lines, every where diverging from the Center of that Circle in which the Attrition is made (or in the Plane of which the Hoop of Threads stands) towards the Circumference of the same Circle; then by the Rarefaction of the Medium contiguous to the Glass, and the necessary Pressure of the more remote and dense Medium, into the Plane of that same Circle, with Directions contrary to those in which the Effluvia are emitted: by this means (I say) the Threads may be regularly directed to the Center of that Circle, in whose Plane the Hoop to which they are fix'd is plac'd.*

For the *Flux of the dense Medium* will be in *Directions contrary* to those according to which the *Rarefaction* is made. But the *Effluvia* are (by the *Hypothesis*) emitted in *Physical Lines, diverging from the Center towards the Circumference*. Therefore the *Rarefaction* of the *adjacent Medium* is according to the *same Directions*. And therefore the *Flux* of the *remote denser Medium*, is in *Lines converging from the Circumference towards the Center*. And all this (by the *Hypothesis*) being in the *Plane of Attrition*; that is, in the *Plane, wherein the Hoop of Threads stands*: therefore the *Threads* are in the *same Plane, wherein the Flux of the dense Medium passes in Lines converging from the Circumference towards the Center*. And therefore by the *Action of the said Medium*, the *Threads may be forc'd into a regular Central Direction*.

Prop. 4. For the same reason; *If the Plane of Attrition be different from that Plane wherein the Threads are*

are

are fix'd; the Threads ought to form themselves into a sort of *conical* Surface; or rather the Surface of a *Trunk* of a *Cone*, whose *Vertex* would be some point in the *Axis* of the *Globe* or *Cylinder*; were the *discharge* of *Electrical* *Matter* every way *equable* and *uniform*. And we find it matter of *Fact*, that the Threads *did actually form themselves into this sort of Figure*.

So that if there were two Hoops of Threads, plac'd one on one side, and t'other of the other side the *Plane of Attrition*, there would be two *Curti-cone* Surfaces form'd; of which the *more Acute* would be that which is *farthest* from the *Plane of Attrition*; and the *more Obtuse*, that which is *nearest* thereto. For when the *Plane of Attrition*, and the *Plane wherein the Threads are plac'd*, do co-incide; then the *Conic Surface* is chang'd into the *Area of a Circle*: because then the Threads lie all in one and the same *Plane*.

Thus much concerning the *Electricity*. I would now subjoyn some few things concerning the *Lights* produc'd in these Experiments.

Prop. 1. Tho' the *Electrical Quality* necessarily requir'd the presence both of the *External* and *Internal Air*, in order to its shewing it self; yet the *Light* requir'd the presence but of one of 'em, *viz.* either the *inward* or the *outward Air*, in order to its appearance.

For either a *Glass Globe full of Air*, rubb'd in *Vacuo*, or with its *Air exhausted*, and rubb'd in *Pleno*, would either way produce a very considerable *Light*.

Prop. 2. There seems therefore to be a real difference between the *Electrical* and *Luminous Effluvia* (at least in some cases :) For by the foregoing *Prop.* these Qualities require different *Circumstances* with respect to the *Circumjacent Medium*, in order to their discovering themselves. And more than that; a *stronger Attrition*, which

generally heightens the Effects of the Electricity, does not at all contribute to the increase of the Light. Nay, Light is producible by the Effluvia of one Glass falling on another; but the Electrical Matter is not to be brought forth, by any such feeble Strokes or Impulses as those are.

Prop. 3. Those Lights (in some Circumstances at least) are less-sensibly affected by the return of the Air, which are produc'd upon an Attrition of exhausted Glass in Pleno, than those produc'd by the Attrition of Glass full of Air in Vacuo.

For, in the former case, no great alteration was found in the Light or Colour, till a certain quantity of Air was let into the inside of the exhausted Glass. But in the latter case, both Light and Colour were sensibly chang'd, at every admission of Air, on the outside of the full Glass.

Prop. 4. Of the various Lights produc'd from various Bodies by Attrition, or (which is equivalent thereto) the Concussion and Agitation of their Parts; some are much more confin'd to a particular Medium, as a necessary Condition of their Appearance, than others are.

That of Culinary Fire, is absolutely limited to such a Medium as Common Air.

Those of Amber, Woollen, Oyster-shells, &c. require a Vacuum, or the nearest approach to it, and utterly disappear in a grosser Medium.

The Mercurial Lights are yet more unlimited, as to the condition of the Medium in which they appear.

For, as they are producible in Vacuo, and in a rarefied Medium approaching thereto; so I have also shewn, That a Light of this kind may be made to appear even in Common Air it self.

And thus much for the Phenomena of Electricity and Light, produc'd by Attrition. From all put together, I hope, something may arise, that may be serviceable to the Design, of gaining some true Knowledge, of the Causes of so surprizing Appearances. And if any one should luckily Improve these short Hints for that purpose, I shall have obtain'd my End.

F I N I S.

PLATE I.

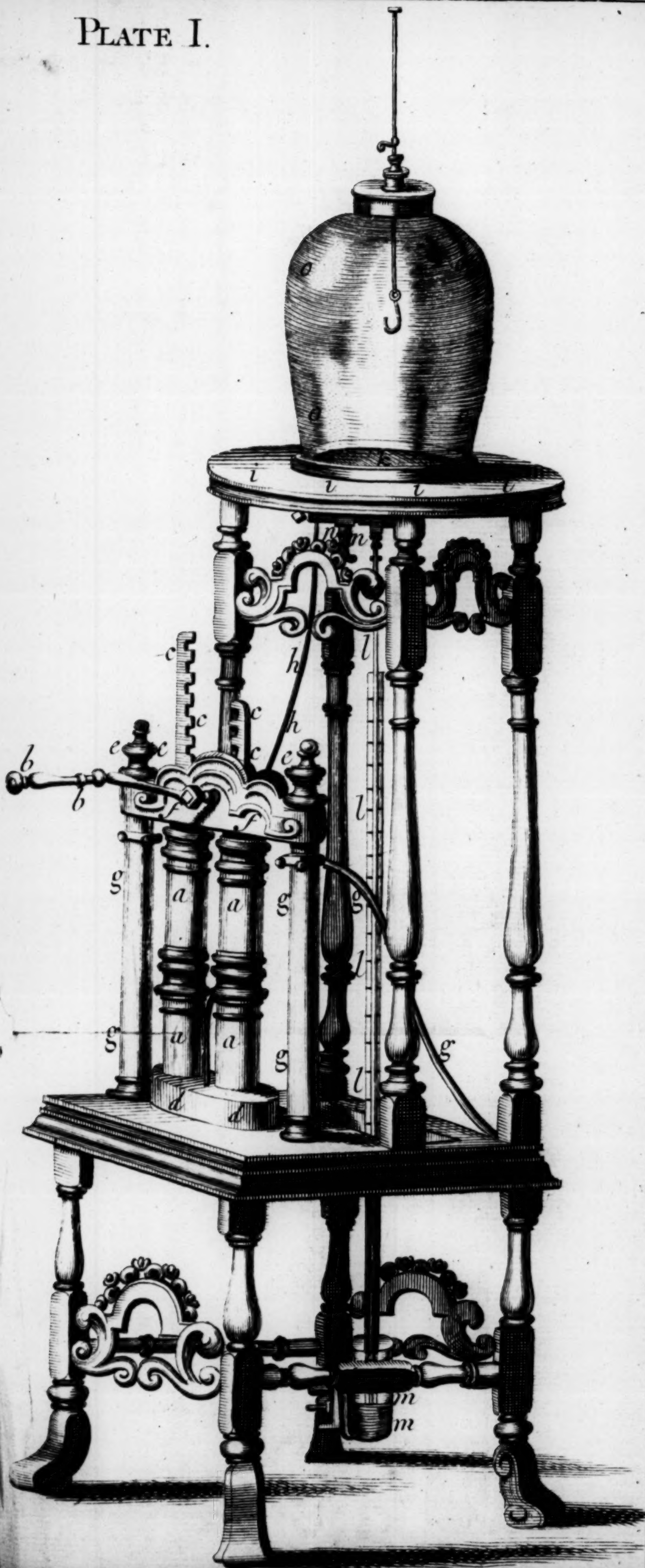


PLATE II.

Fig:

3.

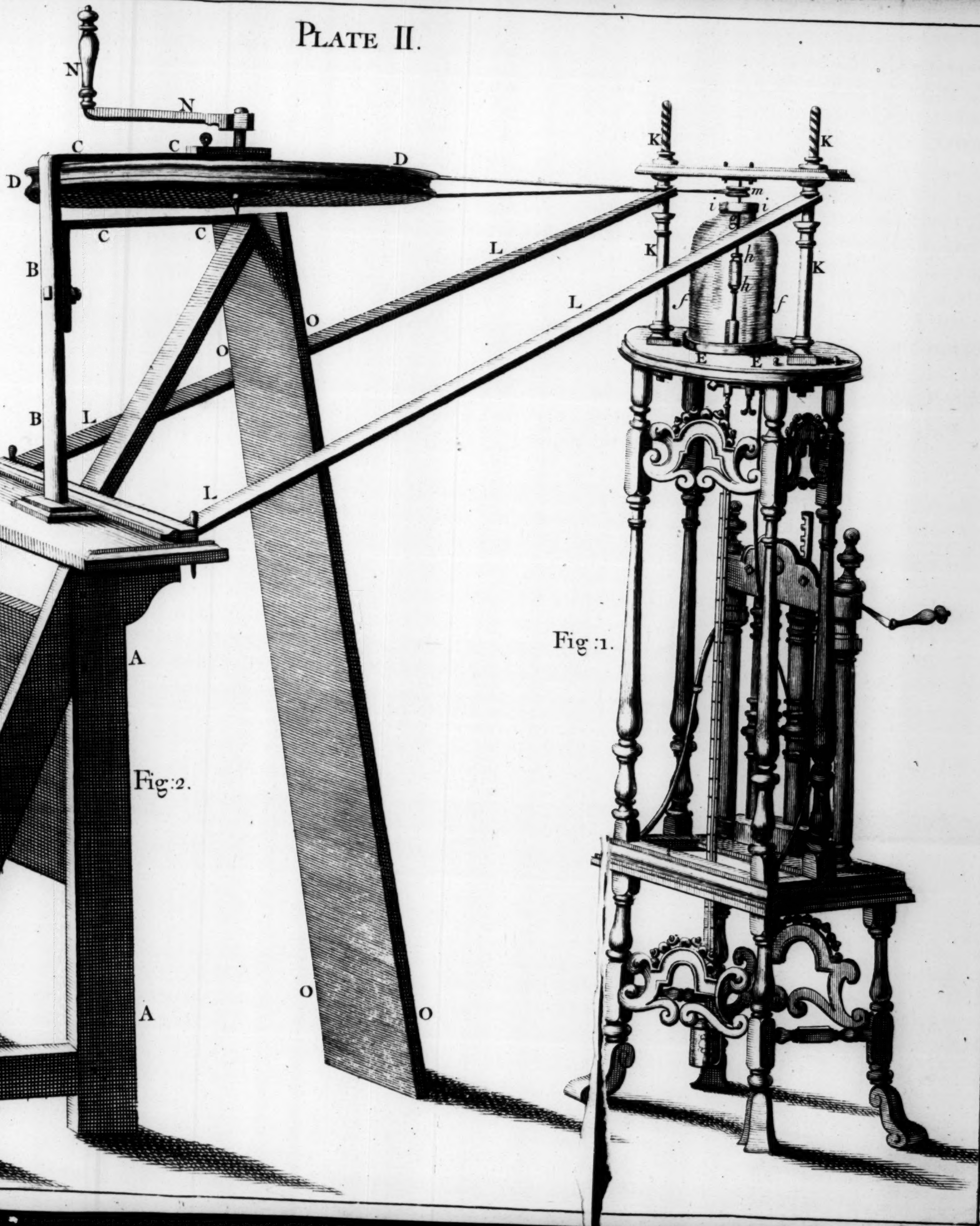


Fig: 1.

Fig: 2.

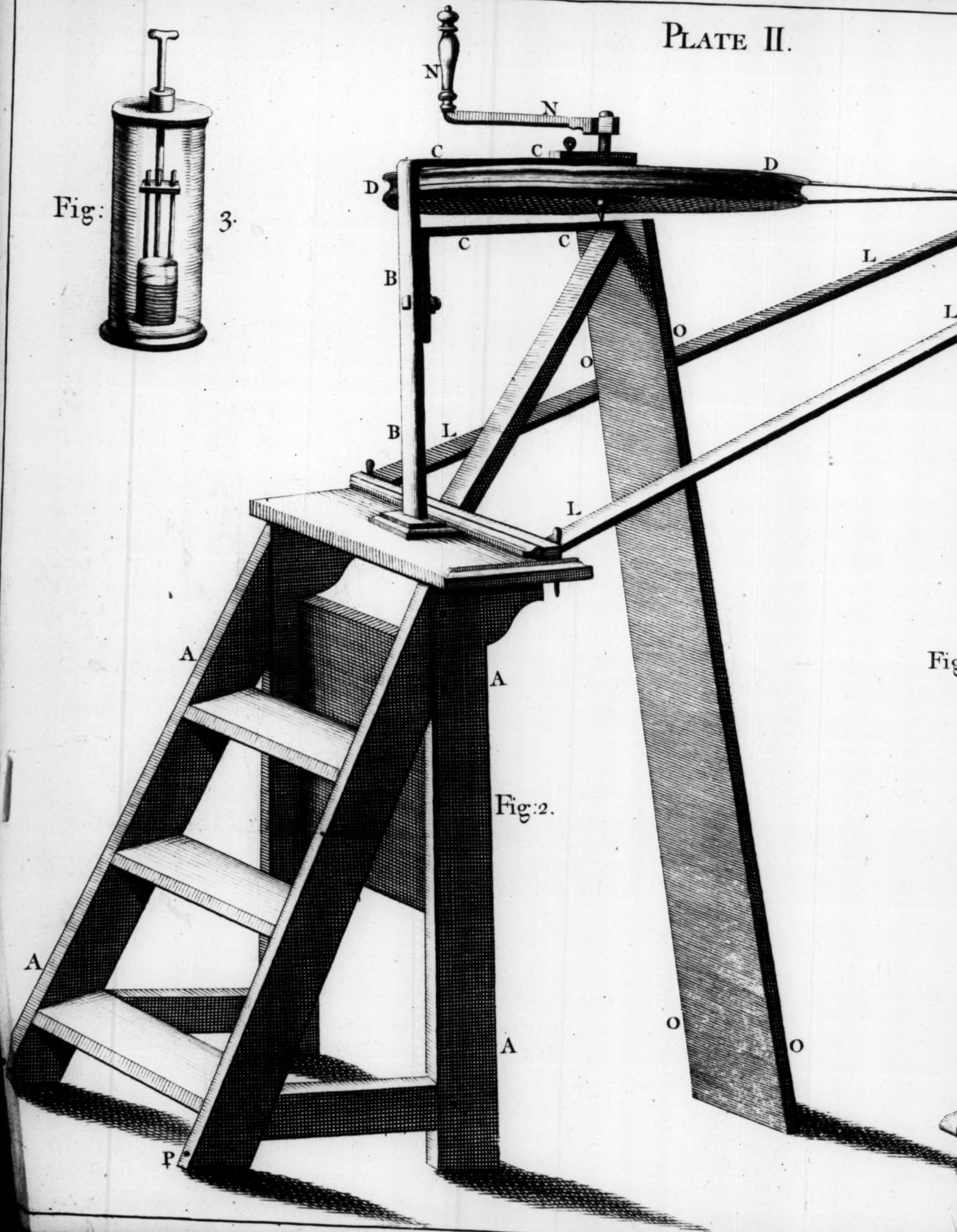


PLATE III.

Fig:6.

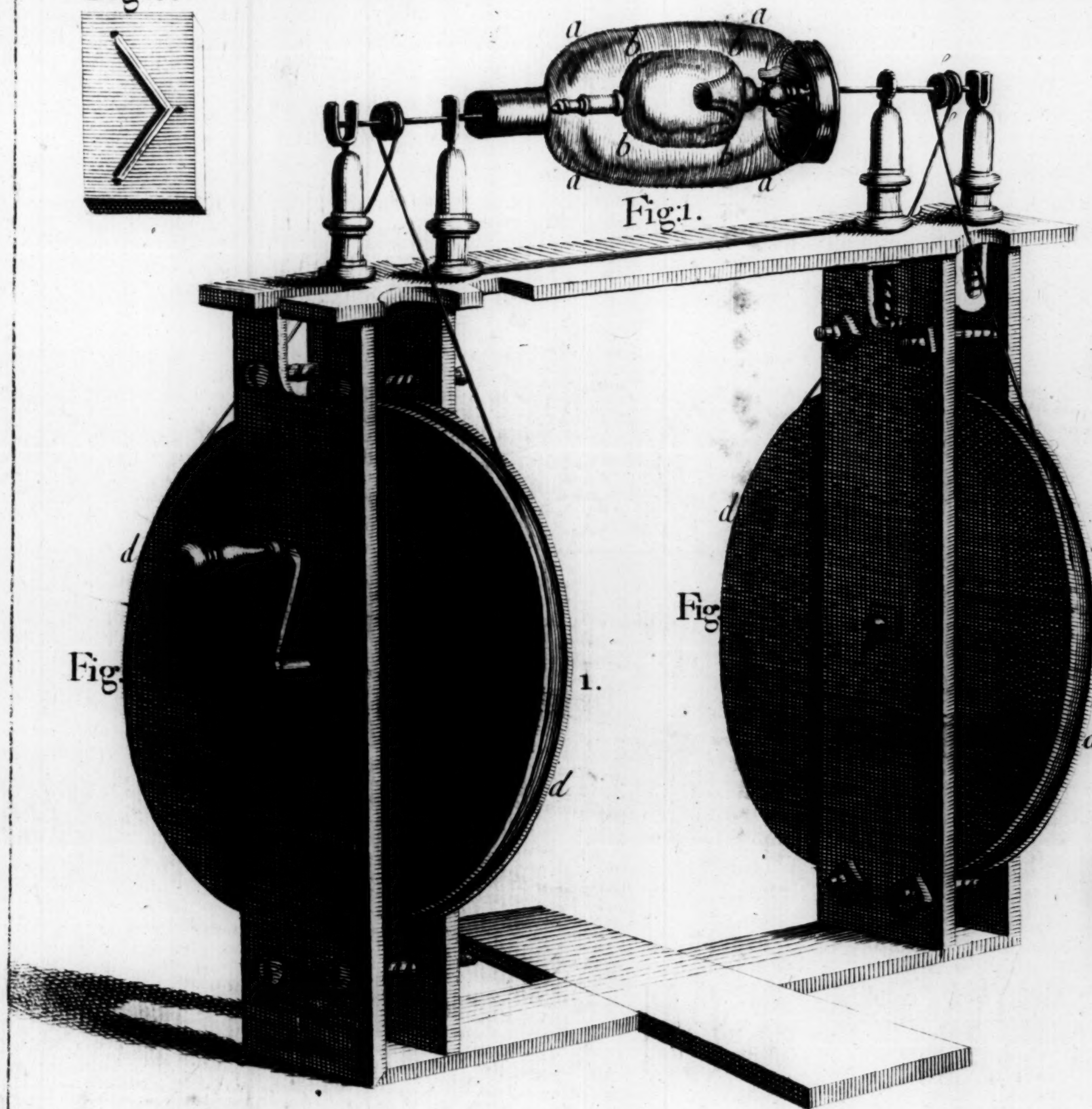


Fig:1.



Fig:5.

PLATE III.

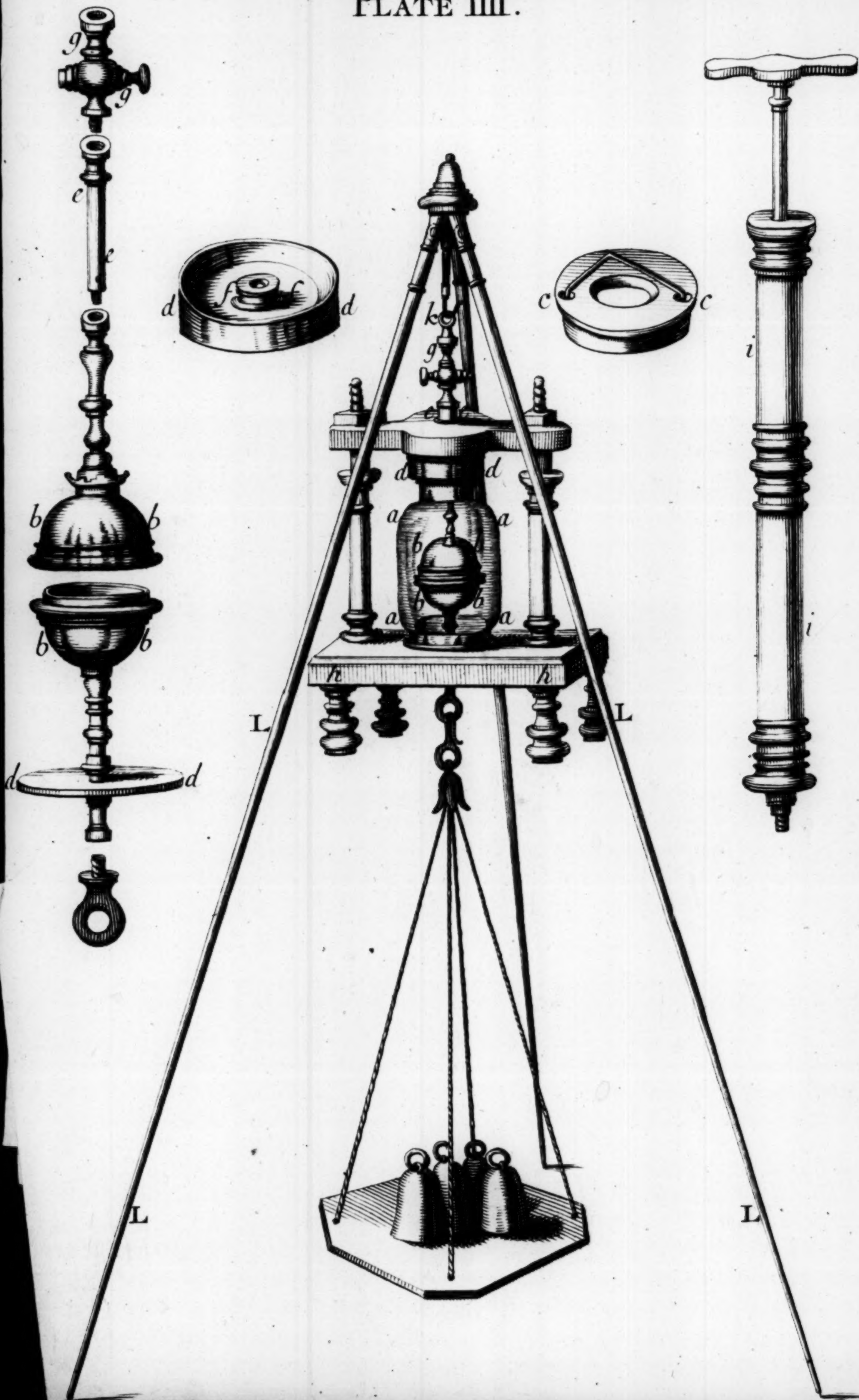


PLATE V.

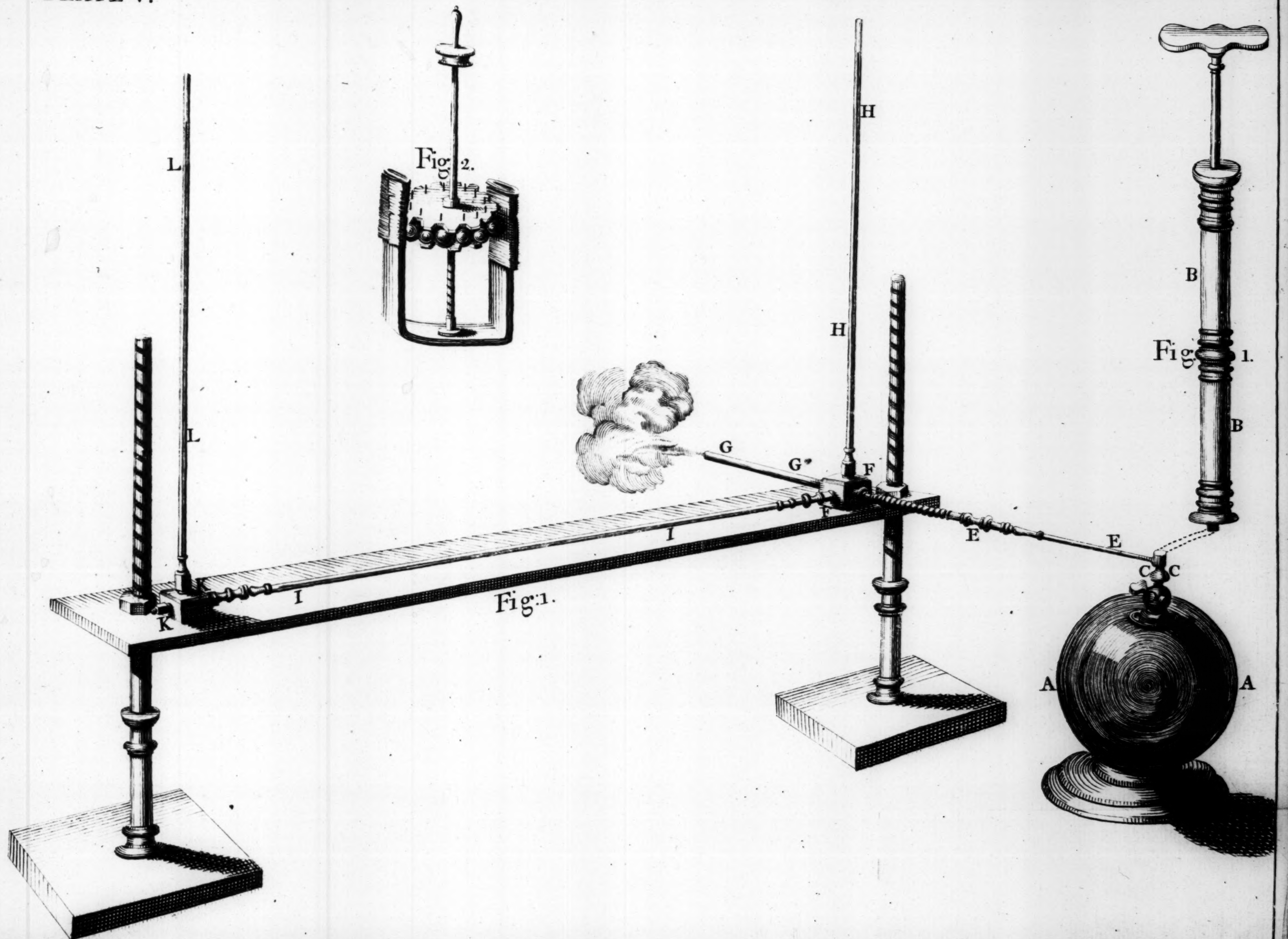


PLATE VI.

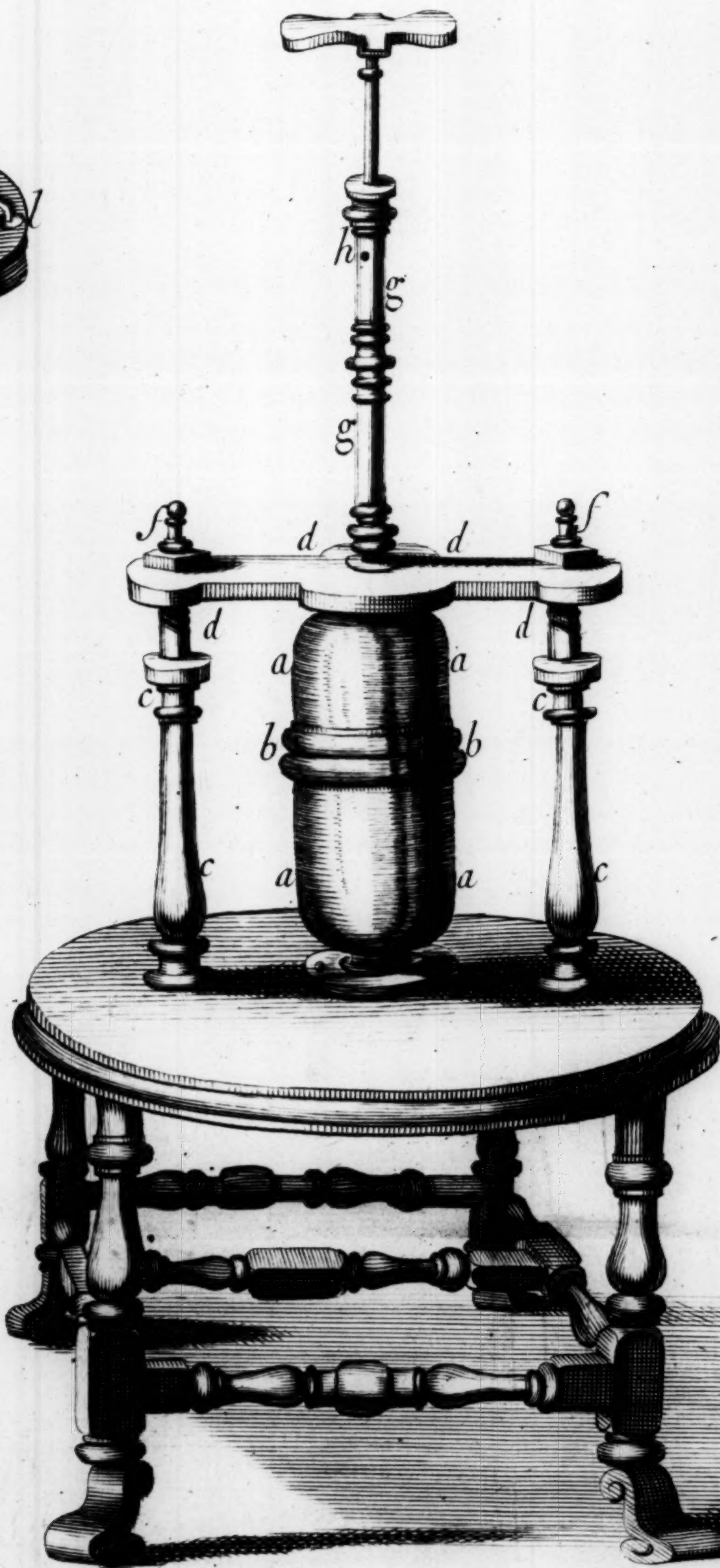
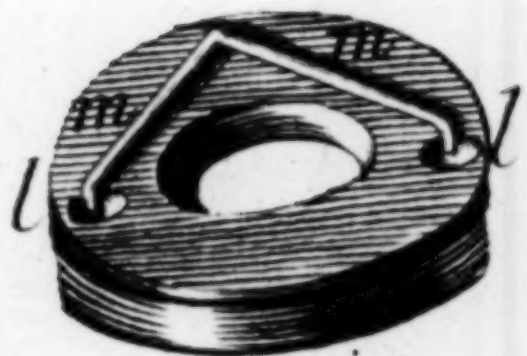


Fig:6.

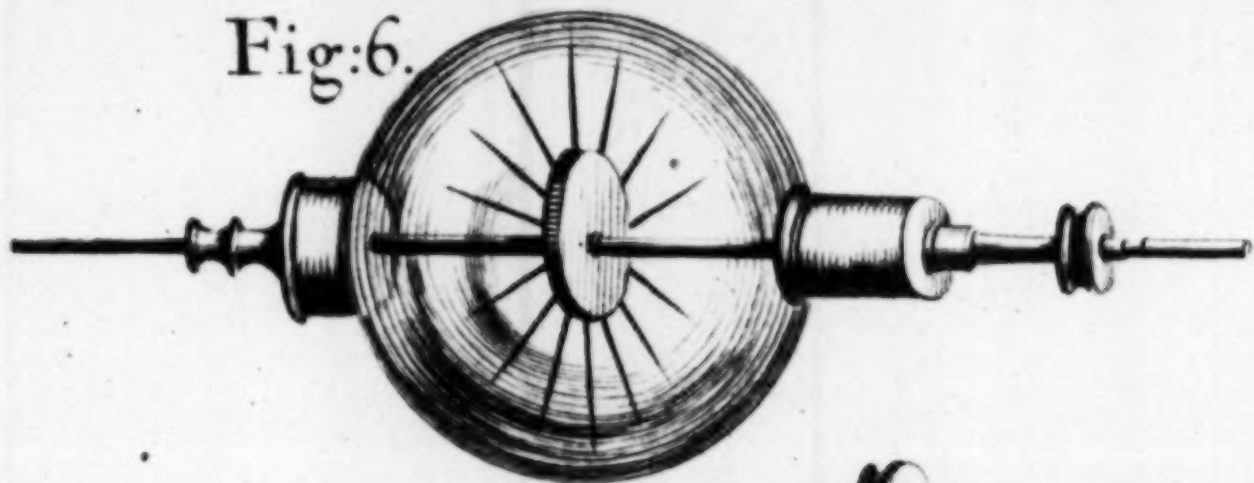


Fig:5.

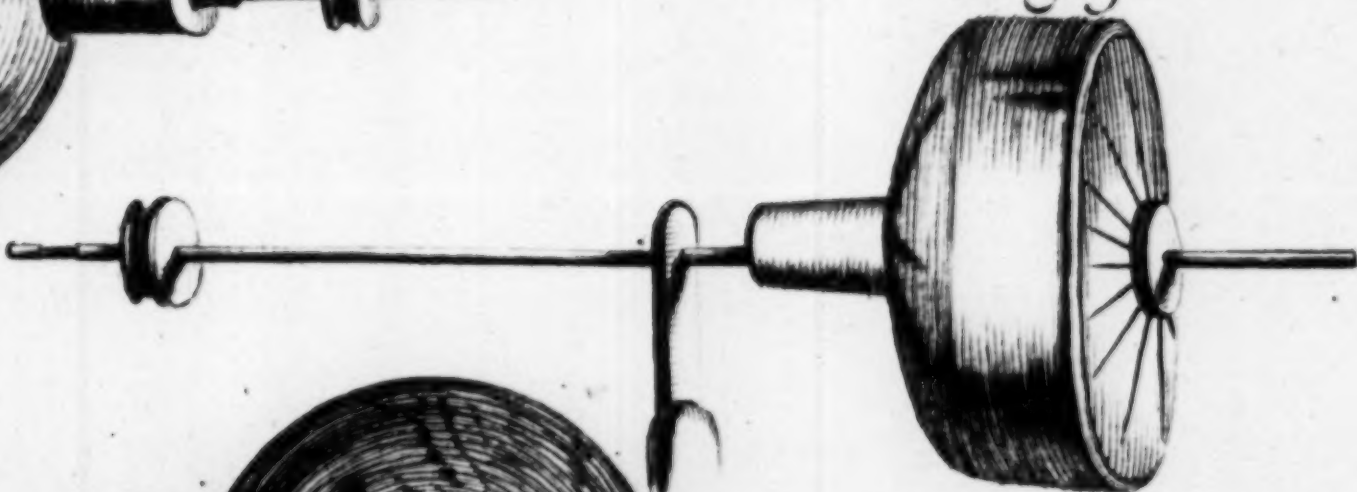


Fig:1.

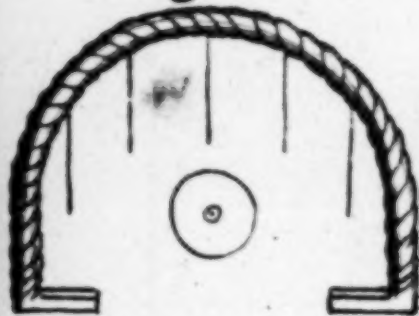


Fig:2.

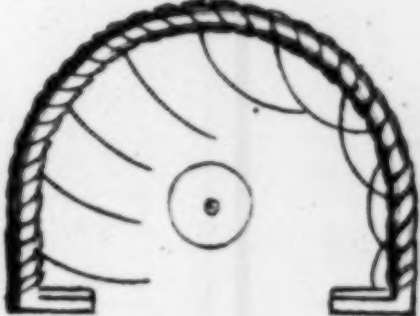


Fig:3.

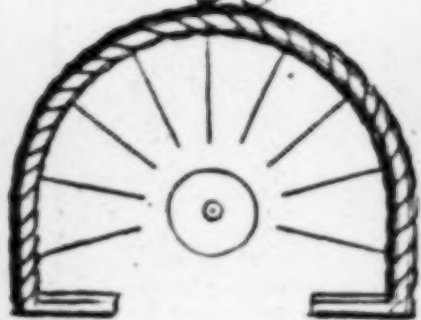


Fig:4.

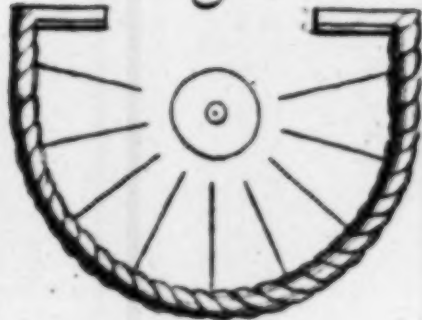


Fig:7.

